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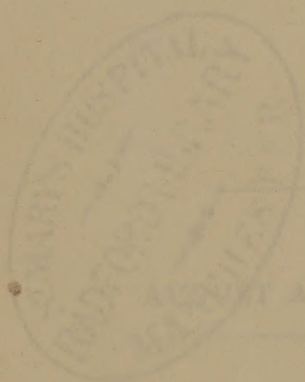
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THE  
DUBLIN QUARTERLY JOURNAL  
OF  
MEDICAL SCIENCE.



VOL. XL.

PART I. JANUARY AND FEBRUARY, 1863.

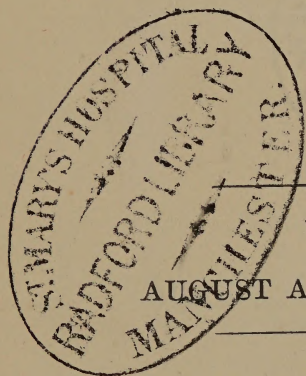
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1863.





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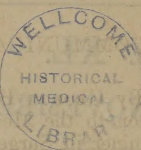
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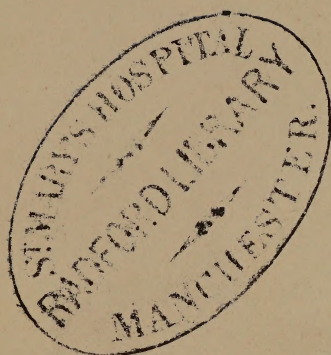


PLATE 1.

Fig 1.



Fig 2.



Fig 3.



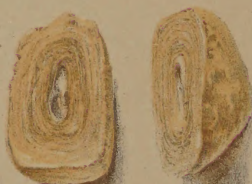
Fig 4.



Fig 5.



Fig 6.





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MEDICAL SCIENCE.

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AUGUST 1, 1865.

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PART I.  
ORIGINAL COMMUNICATIONS.

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ART. I.—*Notes in Practical Surgery.* By GEORGE H. PORTER, M.D., T.C.D.; F.R.C.S.I.; Senior Surgeon to the Meath Hospital and County of Dublin Infirmary; Examiner in Surgery Royal College of Surgeons, Ireland; Consulting Surgeon to the Coombe Lying-in Hospital; Member of Council of the Surgical Society of Ireland; Member of the Pathological Society of Dublin; Lecturer on Clinical Surgery.

1. FOUR SUCCESSFUL CASES OF STONE IN THE URINARY BLADDER REMOVED BY THE OPERATION OF LITHOTOMY.
2. ANEURISM OF THE POPLITEAL ARTERY CURED BY DIGITAL PRESSURE IN TWENTY-SIX HOURS AND FORTY-FIVE MINUTES.
3. CASTRATION; THE VESSELS OF THE CORD SECURED BY ACUPRESSURE.

I HAVE often regretted that the valuable Dublin Hospital Reports have not been continued to the present time in the same able manner in which they were written and published by distinguished members of our profession who are now at rest. I believe that detailed

accounts of cases of operative surgery brought to a successful termination, with a record of the various stages of progress, are invaluable, and will always be perused with interest by the earnest surgeon. It will be admitted also that the professional man who is placed under circumstances which afford opportunities of performing difficult and uncommon operations, is called upon to extend to others whatever experience he acquires. Under this conviction, I have selected from my note book the following cases. Those on lithotomy I consider most noteworthy, because among the diseases to which man is liable there are few which present themselves to the surgeon with more interest than calculus in the bladder. This is not to be wondered at when we take into consideration the extreme sufferings of the patient, which are so frequent and acute as almost to make him wish for death to relieve him. When we remember, moreover, the uncertainty which prevails as to the constituent parts of the stone, and the consequent impossibility of rationally administering medicines with a view to its solution; or when we bear in mind the severe and dangerous operation to which sooner or later the patient is obliged to submit as his only remedy. And as to the operation I may add that, whether we consider its early history, the various modes of performing it, the average of fatal cases in a given number, the dangers attending it, the quickness and brilliancy of its performance, or the relief which it affords in an unerringly fatal disease, lithotomy is one of the most prominent operations in surgery, and gathers round it the largest possible amount of interest for the practical surgeon.

CASE I.—*Man aged Thirty-eight years ; Lateral Operation ; Recovery.*—Thomas D., a farmer, from the County Kildare, was admitted into the Meath Hospital, under my care, Sept. 23rd, 1858. The following is the history of his case as related by himself. About two years previous to his application at the hospital he was suddenly attacked with severe pain in the loins, while engaged at his employment, and immediately afterwards noticed that he passed bloody urine. This subsided in a day or two, and he was then annoyed by pain at the side of his glans penis, with frequent desire to void urine, followed by increased suffering when he had emptied his bladder. He also felt a call to discharge his bowels whenever micturition was demanded. He remained more or less in this condition until about two months prior to admission into hospital, when his sufferings became greatly aggravated. He was then

pressed every half hour to make water, which was always bloody, and with a slightly fetid odour. He could not pass it with any ease unless when lying almost on his face, or partially on his left side; and his sensation was as if his bladder was not quite emptied, followed by painful and continued straining, together with the flow of some drops of pure blood. He described his agony as intense when he sat on the shaft of his car driving. He never had retention of urine, nor did he ever notice that the stream of water stopped suddenly in passing. His health, however, was gradually failing, and he became unable to perform any work. A week before he came to the hospital he could not retain his water, which dribbled away at night, and he was also annoyed by painful erections. When he came under my care all the foregoing symptoms were prominently developed; and on examination it was found that he had a large bunch of external hemorrhoids, and that the glans penis, which was quite uncovered by prepuce, was looking moist and excoriated, and the lips and orifice of the urethra red and slightly everted. On the 21st September, with the assistance of my colleagues, I sounded him, being unable to pass a sound larger than No. 9, in consequence of the small size of his urethra, which was throughout narrow, though no abrupt stricture existed. The instrument on its introduction caused him a great amount of pain, but it immediately struck the calculus, giving proof of its presence. I withdrew the sound, and endeavoured to measure the stone with Mr. L'Estrange's ingeniously-constructed lithometre, but I was disappointed, as I could not grasp it. The sensation, however, which was given from passing the instrument over the surface of the stone led to the impression that it was of considerable dimensions. His urine was acid, and contained some pus globules. It also became slightly clouded when heat was applied, but only to as great an extent as could be explained by the presence of the purulent matter. On the 1st October, having his rectum cleared by an enema, and himself placed under the influence of chloroform, I proceeded to cut him for the stone. The man having been tied in the usual manner, I injected three ounces of tepid water into his bladder, and a No. 9 staff, grooved on the side, having been introduced, and admirably held by the late Mr. Smyly, the patient's buttocks were brought a little over the edge of the table. Kneeling then on my right knee, I commenced an incision with Mr. Ferguson's knife one inch and three quarters in front of the anus, to the left of the raphe; this I carried down midway between the tuberosity



of the ischium and the anus, fairly passing both. By so doing it will be evident that the length of the cut was considerable; and I deem it of great importance to make the first incision a free one; (nearly three inches in the adult). I then divided the deeper structures with one or two light strokes of the knife; and having felt for the groove of the staff with the forefinger of my left hand, the nail of which I firmly fixed in it, lowering my wrist I thrust the lateralized knife from the *lowest part* of the wound upwards, and lodged its point in the groove, transfixing the urethra at its membranous portion. I then steadily pushed on the knife along the groove into the bladder, dividing partially the triangular fascia, levator ani, prostate, and neck of bladder. A small quantity of water now escaped along the knife, when I rapidly withdrew the instrument, slightly making it cut its way out. I now laid aside Fergusson's knife, and passed Crampton's lithotome along the groove of the staff into the bladder, and carefully sent the index finger of my left hand along its back, and thus cut the prostate to a sufficient extent. Having withdrawn the lithotome, the cutting part of the operation was concluded. Keeping the index finger of my left hand in the wound and bladder I turned it twice round to dilate the parts, and then, having felt the stone, desired the staff to be removed. On the finger so placed I introduced a blunt gorget, along which, when I withdrew my finger, a large gush of water flowed. I then, guided by the gorget, introduced the forceps, took out the former, and keeping the forceps closed until I felt the stone, opened its blades, seized, and extracted the calculus. I then sought to ascertain if a second stone was present, but not finding any, I examined the wound, and there was no bleeding. I at once introduced, and fastened in, a tube, placed a suppository containing a quarter of a grain of morphine in his rectum, and caused the patient to be untied and removed to bed. It would be tedious to note from day to day the changes in this case. His progress to perfect recovery was rapid, and no bad symptom occurred throughout. The stone weighed in air 688 grains, in water 353 grains. Specific gravity 1.938. A tolerably faithful drawing of it is given in Plate I., Fig. 1. The man left the hospital, perfectly cured, on the 27th of November.

CASE II.—*A Boy aged Six years; Lateral Operation; Recovery.*—Patrick G. was admitted into the Meath Hospital, May 20th, 1862, suffering from symptoms of stone in the bladder, such as very

frequent calls to pass water for the preceding twelvemonths, great pain after the bladder was evacuated, and at times, prolapsus ani when straining to void urine. He was constantly pulling at his prepuce, which was much elongated, and slightly excoriated. He complained occasionally of severe pain in the lower part of the abdomen, and used to expel large quantities of flatus. His bowels were at times very loose, and he often picked his nose until it bled. There never was any blood noticed in his urine, nor did the stream of water ever stop suddenly, but at times it passed in a quivering interrupted stream. On the 21st May I had this boy chloroformed, and sounded him; but did not detect the presence of the stone. I had his pelvis then raised at least four inches above the level of his shoulders when the sound struck the foreign body, and declared its presence in the bladder.

*Operation.*—May 30th. His rectum having been cleared out by an enema of thin gruel in the morning, he was chloroformed, and two ounces of tepid water were thrown into the bladder by means of a catheter and syringe. A staff, laterally grooved, of the largest size his urethra would allow was introduced, and intrusted to the care of Mr. Wharton, who held it up well to the arch of the pubis, and he was tied in the usual manner. During a spasm some of the water escaped, and prolapsus ani occurred, but the bowel was with facility returned, and by gentle pressure remained in its place. I commenced an incision with Fergusson's knife, at the left of the raphe, about three-quarters of an inch in front of the anus, and carried it backwards to the extent of two inches. I then gave the deeper structures one stroke of the knife; and having felt for the groove of the staff, I fixed the nail of the left hand fore-finger in it, and immediately struck upwards with the point of the knife lateralized for the staff. Having placed the knife safely in the groove, I moved it from side to side, making sure that it was in its proper position. I then pushed it on to the bladder, dividing the neck of it, and the little prostate; withdrawing the knife, I gently inserted the fore-finger of my left hand, felt the stone, and directed the staff to be removed. On my finger I introduced a small gorget, and along it a forceps, grasped the calculus, and delivered it. I then felt for a second stone, but none being present, I examined for bleeding, and found some slight oozing, but no hemorrhage from any particular point. The *canule à chemise*, well oiled, was then inserted. The patient was at once placed in bed, with a waterproof sheet, and a folded blanket under his hips.

May 31st.—He had passed a tranquil night, and the water flowed freely by the canula. Daily the little fellow improved in health; the urine passed freely through the penis on the sixth day after the operation, and he left the hospital cured, August 6th.

My friend Professor Haughton kindly examined the calculus, a drawing of which I have added, Plate I, Fig. 3, when the following was the result:—

Weight in air, 328·3 grains.

Weight in water, 130·1 grains.

Specific gravity = 1·656.

The central nucleus was  $\frac{1}{4}$ th the linear diameter of the calculus, = to  $\frac{1}{64}$ th of its entire bulk, and composed altogether of oxalate of lime.

The exterior portion of the stone was concentrically arranged, porous, composed of lithic acid, with small portions of lithate of ammonia, phosphates of lime, and magnesia

CASE III.—*Man aged Twenty-five Years; Lateral Operation; Recovery.*—John M'N., a baker, was sent to me from the county Tyrone, and admitted into the Meath Hospital, September 12th, 1864. The following were the symptoms under which he laboured:—Frequent inclination to make water, and intolerable pain when the last drops were being expelled; a dull pain in the lower part of the abdomen; occasional attacks of tenesmus. He was very much emaciated, and his countenance indicated suffering. He had symptoms of irritation of the bladder for two years previously, but never had retention of urine, or sudden stoppage of the stream. I sounded him, and with ease detected the calculus. The examination gave a good deal of pain, and some blood followed. He was ordered a hip-bath, and a mixture containing camphor and hyoscyamus, and the next day I found him easy.

*Operation.*—September 27th.—At 7 o'clock, a.m., he got an enema, which washed out his rectum thoroughly, his perineum was shaved, and at ten o'clock I had him brought into the operating theatre. I then passed a catheter, and drew off urine, and immediately afterwards injected about five ounces of tepid water. He was then put under chloroform, and whilst this was being accomplished, he was tied, and a good-sized staff, grooved on the side, introduced. With the staff he was sounded, the stone was felt, and the instrument was intrusted to my colleague, Mr. Wharton. His buttocks having been placed a little over the edge of the table,



and held perfectly straight before me, I commenced an incision with Fergusson's knife, to the left of the raphe, about one inch and three-quarters anterior to the anus, and carried it backwards half way between the tuberosity of the ischium and the anus. I then went deeper, by two or three touches of the knife, over the same track. I felt with the left fore-finger for the staff, and having fixed my nail in it, lowering my hand, and lateralizing the blade of the knife, I struck upwards, and lodged the point in the groove, and pushed the knife along it into the bladder, until I felt it arrested by the stop at the end of the staff. A little water flowed at this stage of the operation. Laying aside the knife, I passed in the lithotome, and my left fore-finger was inserted after it along its back, and divided the prostate; I then withdrew it, still keeping my finger in the bladder, with which I could distinctly feel the stone. I then requested the withdrawal of the staff, and along my finger passed in a gorget. A copious flow of water now coursed along the instrument. I introduced the forceps, laid down the gorget, seized a stone, and extracted it. A facet was observed on the calculus; and on introducing my finger, a second one was detected. The forceps was again introduced on my finger, and the other stone delivered. I now examined for bleeding, and, as there was none of consequence, I introduced a tube, tied it into the bladder, placed a morphine suppository in the rectum, had him untied, and conveyed to bed.

Sep. 28th.—He had passed a good night, the urine coming freely through the tube and wound.

Sep. 29th.—Felt a little feverish to-day; there was no pain; the stomach inclined to be sick. Ordered him small effervescing draughts, containing tincture of opium, and ice.

Sep. 30th.—Much better this day; he had a good night, free from all pain. I then removed the tube. This man progressed most favourably to recovery. The urine passed through the penis on the seventh day after the cutting, and he was discharged cured on the 5th December.

Figures 4, 5, and 6, Plate I., give very accurate representations of these stones. Fig. 4 exhibits the facet, Fig. 5 the manner in which they lay together in the bladder, and Fig. 6 a section which shows a cavity in the centre, proving that the nucleus was organic, and not from the kidney. I am indebted to Professor Haughton for the following qualitative analysis:—

Composition — Principally phosphate of lime, with traces of

phosphate of magnesia and lithates. They are rather rare urinary calculi, as they consisted so much of phosphate of lime.

CASE IV.—*A Youth aged Sixteen Years ; Lateral Operation ; Recovery.*—Christopher C., a native of the County Wicklow, was sent to be placed under my care, and admitted into the Meath Hospital November 3rd, 1864. On his admission, all the rational symptoms of stone in the bladder were present. Assisted by my colleagues, I sounded him the day after he came in, and instantly detected the presence of a calculus. He had suffered for two years from vesical irritation, but the last two months before he came into hospital the pain he felt after micturition was most poignant. I had him placed in a warm bath at night on four different occasions, and gave him medicine likely to soothe the irritable condition of his bladder. On the 16th of November I proceeded to perform the operation of lithotomy for his relief. His rectum having been thoroughly evacuated by a simple enema, I had him brought under the influence of chloroform, and tied on the table of the operation theatre. I injected about four ounces of water into his bladder, having previously drawn off all urine. Having done this, I at once introduced a large-sized staff, grooved on its side, into the bladder, and with it sounded for, and felt the stone. And this is a preliminary never to be forgotten by the surgeon. It is no rule of my laying down, for every good lithotomist has enforced it. How fearful would be the mistake of cutting into a bladder and finding it free from the presence of the stone which, the very day before, had been distinctly felt! The possibility of passing one of small dimensions must never be forgotten. Convinced, in this case, of the existence of the calculus, I knelt on my right knee, laid Fergusson's lithotomy knife on his perineum, a little to the left of the raphe, and carried it back well beyond the anus, midway between it and the tuberosity of the ischium. Having deepened this incision sufficiently to fix the nail of my left fore-finger in the groove of the staff, I thrust the knife, lateralized, from the lowest part of the wound upwards and backwards, and transfixed the membranous portion of the urethra in the manner already described in the three former operations, and concluded the extraction of the stone in the same way. Assured by a careful examination that no second calculus was present, I looked for bleeding; and as none was visible, I put a tube in the wound, tied it in its position, and had the boy carried to his bed, which was prepared with a waterproof

sheet, and folded blanket beneath his buttocks. The only peculiar feature in the operation worth noticing was the fact that I caught the stone in the long diameter, and was in consequence obliged to use a good deal of force with an up and down motion of the handles of the forceps before it was completely delivered. This lad recovered most favourably. From the day of the operation he steadily improved, without the supervention of any unfavourable symptom, and left the institution, cured, on the 2nd January, 1865. A very faithful coloured lithograph of this calculus is given in Plate I., Fig. 2.

The practical points that I would urge on those who may be called on to perform the operation of lithotomy, are mainly with reference to the form of the staff, the injection of the bladder, and the manner of opening the urethra prior to cutting the prostate gland.

Differently-shaped staffs or guides into the bladder have been recommended to the surgeon who cuts for stone. Amongst those most lauded I may mention the ordinary one grooved on its convexity, the almost straight staff of Key, the rectangular of Buchanan, and Liston's, grooved on the side. The last named I have always used myself, and have no hesitation in giving my opinion in its favour, as possessing superior advantages. I believe the side-grooved staff is more easily struck, or reached, than those which are grooved on the convex part. But a still more important point in this construction is, that it more or less enforces the lateralization of the knife on its way into the bladder, thus keeping off its edge from the rectum. I have never seen this bowel wounded when Liston's staff was employed.

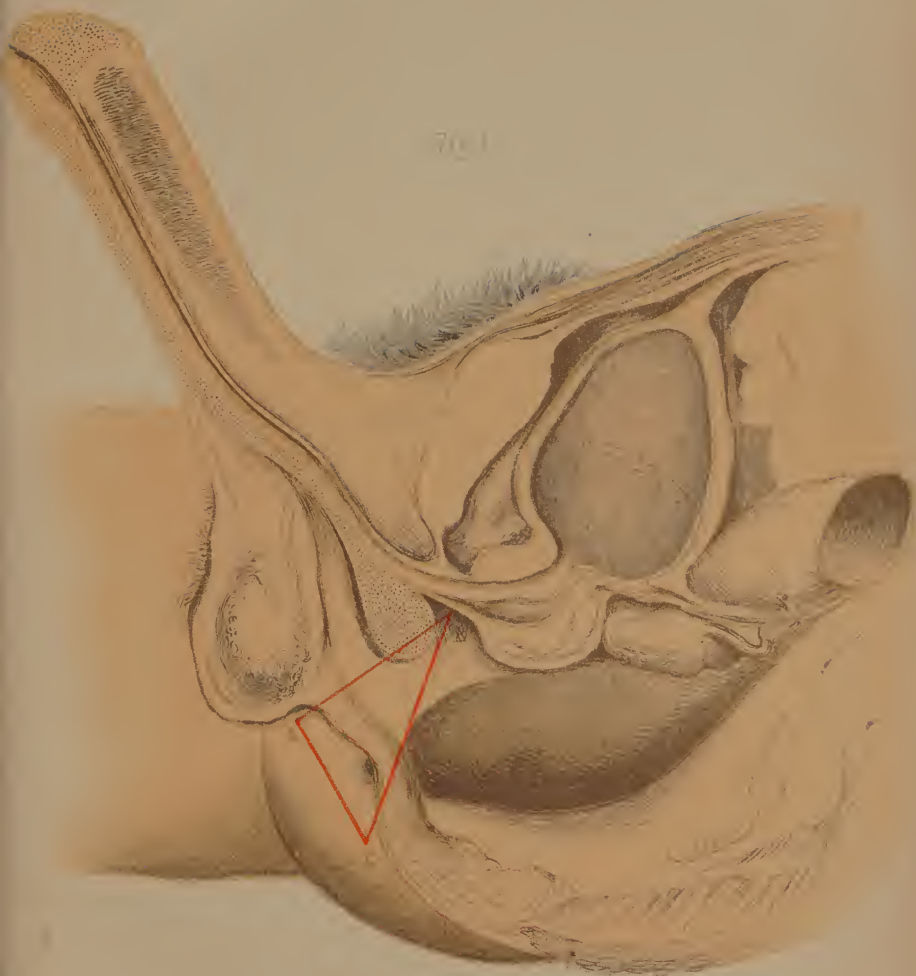
With regard to injecting the bladder before the operation, I must lightly approach this question. Some surgeons believe that an empty condition of the viscus is to be preferred, because it allows the stone to lie close to the neck of the bladder. On the other hand, it has been recommended to permit urine to collect for an hour or two previous to cutting. I must say that I consider it is far better to cut upon a bladder containing fluid, than when it is in an empty and collapsed state. It is more in its proper position when moderately distended; it is also more steady and resisting to be cut into, and the gush of fluid unmistakably proclaims that the operator has driven his knife in the right course. But it is better to evacuate all urine, and to throw tepid water into its place. A great deal has been written concerning the danger of infiltration of urine



as a consequence of lithotomy, and much stress has been laid on the extent to which the prostate gland should be divided in order to obviate this unhappy occurrence. I feel convinced that too much credence has been given to this notion concerning the division of the gland beyond the limits of its capsule. Of course this division should be avoided, and it is not necessary unless the stone be of enormous size. My impression is (but I by no means put it forward as a positive doctrine) that infiltration is often produced by a want of correspondence in the different incisions as the knife travels to the bladder, and that it occurs almost immediately with the first escape of urine. Every surgeon who has seen cases of extravasation of urine from ruptured or lacerated urethra, knows its baneful effects on the parts it touches, and how quickly it begins its action, ending in death of those parts. Now it strikes me that should the incisions not correspond, or be carried beyond the proper extent in the prostate, the contact of tepid water is safer than urine; and that the introduction of a tube into the incision and bladder, before the urine can be collected in any quantity, may possibly avert the dangerous consequences of a sudden gush of this secretion into the parts around the wound.

The last point on which I would dwell is the manner in which the operator should direct his knife when striking for the staff. The part of the urethra that should be opened is the membranous; and if for a moment its position be considered, overlapped more or less by the bulb, it will be seen that it is by no means an easy procedure to transfix it without wounding the bulb, and its artery. No doubt it has often been laid down by excellent surgeons that it is almost impossible to avoid wounding the artery of the bulb, and that such injury is of little moment. I maintain, nevertheless, that, if possible, such should not be done. To the very young or old patient the loss of blood is always a source of danger; and even regarding bleeding from the artery of the bulb in the lightest manner, it must be allowed that it is a most disagreeable and unhappy occurrence. I conceive that the operator may avoid this if, when striking for the staff, he directs his knife, properly lateralized, upwards and backwards from the *lowest* part of the first incision. Plate II. will explain my meaning. It will be seen that the wound in lithotomy is triangular in shape, the apex, B, at the membranous part of the urethra, and the base formed by the external incision, A C. It is quite evident, looking at this, that a line of incision passing to the membranous part of the urethra, from any

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part of the wound except the lowest, must strike the bulb, but that if the knife be directed up from the lowest part it passes behind this overlapping portion. In order to accomplish this part of the operation it is necessary to lower the wrist when making for the staff, and therefore I consider the kneeling posture the one which brings the operator well under the bladder, and enables him most easily to perform this important step of his operation. I have no doubt but that this procedure also materially aids in the prevention of infiltration of urine, by making the wound into the bladder more direct and depending.

## 2.—ANEURISM OF THE POPLITEAL ARTERY CURED BY DIGITAL PRESSURE IN TWENTY-SIX HOURS AND FORTY-FIVE MINUTES.

The cure of aneurism by any form of compression will always be regarded with considerable interest by the Irish surgeon, as among the many improvements in modern surgery, and as one which we are entitled to claim as essentially our own. The simple, and almost painless, pressure of the human finger, producing a rapid and easy cure of a popliteal aneurism, is well exemplified, I conceive, in the following case, which I had under my care in the Meath Hospital.

George K., aged sixty-six years, was admitted to the hospital April 18th, 1865. The history of his case was briefly as follows:—Five years of his life, up to ten months prior to seeking advice, he had been employed driving a car. The seat from which he drove he described as being too low for a man of his height, and causing his knees to be bent and cramped considerably. His employer's death, however, compelled him to give up this occupation and adopt that of a labourer, when he had occasionally to walk great distances. Towards the end of December, 1864, he suffered greatly from a cramp which extended down the back of his right leg, from the knee to the heel. He did not, however, at that time seek any remedy, but endured the pain until the middle of February, 1865, when he noticed a pulsating tumour in the right ham. He was annoyed by feeling a throbbing in the part, and stiffness, particularly after sitting a short time. On the day mentioned he sought relief for a hydrocele of the tunica vaginalis testis, and complained of a swelling, and throbbing behind his right knee. The main symptom present on admission was a pulsating tumour, the size of a small hen egg, in the right popliteal space, and the circumference

of the right limb (measuring over the centre of the tumour), was one inch larger than the left. The tumour became smaller and softer when pressure was made upon the femoral artery at the groin, and at the same time all pulsation ceased. The pulsation was eccentric and synchronous with that of the heart, and a *bruit de soufflet* was audible on applying a stethoscope to the tumour. There was not the least amount of œdema of the right leg or foot observable. The patient was a good-tempered old man, and when the nature of his malady was explained, and the mode of treatment mentioned, he at once promised to aid in every way by obedience to my orders. On a careful examination of his chest and abdomen no other aneurismal development could be discovered. Considering this a very suitable case to try digital pressure, I commenced it on the morning of the 19th April, at 11 o'clock, a.m., a number of the pupils of the hospital kindly volunteering their assistance, and each gentleman becoming responsible for two hours pressure at a time, up to 9 o'clock, p.m. Having applied a flannel roller from the foot to the knee, and shown the pupils how to make pressure by placing the points of the middle and index fingers of the left hand over the femoral artery where it crosses the brim of the pelvis, then to fix and increase the pressure by placing the middle and ring fingers of the right hand on those already over the vessel, I desired that the pulsation in the tumour should be arrested as much as possible. I also taught the patient himself to press the vessel, and throughout the treatment of the case he occasionally gave assistance to the young gentlemen in charge. I visited him at 2 o'clock, p.m., April the 19th, and learned that the compression had been carefully sustained, and was well borne by the patient. The tumour was now certainly more solid than before, but the pulsation remained undiminished whilst the leg and foot affected were warmer than the other. Again, at 5 o'clock, p.m., of the same day I saw him. The solidity of the aneurism had by this time evidently increased; but as yet no apparent change had taken place in the force of pulsation. 9 o'clock, p.m. The tumour now was much more solid. The pressure was then discontinued for the night, and thirty drops of Battley's sedative liquor of opium administered in a draught.

April 20th, 9:30 o'clock, a.m.—The man had passed a tolerably good night; awoke once about midnight with a sharp pain in the tumour. This was the first time he had ever felt pain in the aneurism, but it did not last more than half an hour, when he fell asleep. Compression was resumed this day, by gentlemen who

came forward to offer their assistance, as others had done on the day before. 12 o'clock, p.m.—The tumour now was very solid, and the pulsation was greatly diminished. 1.45 o'clock, p.m.—Pulsation had now ceased completely, and the sac was firm, and solid. The patient complained of slight numbness along the back, and outside of his leg, but there was no œdema whatever in leg or foot. The pressure was continued in a slight degree up to 7 o'clock that evening, when it was finally discontinued. The beating of the femoral artery could be traced down at this period to about four inches from the knee. He got the opiate, as before, at 9 o'clock, p.m.

April 21st.—He had passed an excellent night; no pain or swelling in the limb. The superior internal articular artery was evidently enlarged, and pulsating strongly. I kept him in bed for a fortnight from this date, with the limb rolled in flannel, and the tumour gently rubbed, every night, with a liniment, consisting of 2 oz. of soap liniment and 15 grains of iodide of potassium. He was then allowed to get up, and move about the ward on crutches.

May 29th.—I tapped his hydrocele; and on the 3rd of June he was discharged the Hospital.

Thus, in the short time mentioned, this formidable disease was perfectly cured by finger pressure. The point where the fingers rested was only slightly reddened, but did not become sore throughout the treatment; and he never complained that the pressure gave him pain. This contrasts strongly with instrumental arrest of the flow of blood through an aneurism; for no matter how carefully applied, it cannot be borne so long as can the finger top; and further, the artery cannot be isolated so completely from its accompanying vein by mechanical as by digital compression, and therefore, œdema of the limb is more likely to ensue. There is less likelihood of ulceration, or sloughing of the skin occurring under the point of pressure than when an instrument is used, inasmuch as by being so direct, a smaller amount of force suffices to control the current of blood through the artery; and, as the point of pressure can with such ease be changed to a higher or lower position. But in addition to all this, the rapidity of the cure in cases of digital treatment contrasts most favourably with any mechanical pressure, as in one case a few hours usually accomplish what in the latter requires days. With respect to the rapidity of cure in cases of aneurism treated by compression of any kind, I cannot more appropriately conclude this account of mine, than by citing some remarks of



my esteemed friend and very accomplished surgeon, Mr. L'Estrange, at a recent meeting of the Surgical Society of Ireland. They struck me forcibly at the time, and are well worth the attentive consideration of every practical surgeon. I quote from the *Dublin Medical Press*, June 7th, 1865, p. 536. He said:—"I would wish to observe that it appears to me that there must be some peculiar state of the blood, which admits of quicker coagulation in some individuals than in others, placed under similar circumstances and under similar treatment. For instance, we have in this city, within these last few months, three cases of aneurism treated by external compression. In one, cure by coagulation of the contents of the sac was effected in the short space of five hours and three-quarters (Dr. Mapother); the second was effected in twenty-five hours and a-half (Mr. Porter)—(here he slightly erred); and in the third, coagulation could not be effected although the external compression was steadily kept up, and well borne by the patient for many weeks. It would be of great importance to ascertain this physiological and pathological fact, as to the coagulation of the blood in the aneurismal sac under compression. This, I should think might be ascertained, or great light thrown on the subject, by taking a small quantity of blood from each of these patients—Dr. Mapother's, Mr. Porter's, and the one in the City of Dublin Hospital—in which coagulation would not take place by external compression; and by submitting their blood to chemical and microscopical tests, much information might be obtained, which would conduce materially to the scientific treatment of aneurism."

### 3.—CASTRATION; THE VESSELS OF THE CORD SECURED BY ACUPRESSURE.

This new mode of arresting hemorrhage has not yet been much adopted in this country, although emanating from the brilliant genius of Professor Simpson, whose name must give weight to any medical or surgical suggestion. In Dublin, the first surgeon who used acupressure, according to this modern plan of Simpson, was my esteemed colleague, Professor Macnamara, in a case of amputation of the breast, and with perfect success. He has also by this means arrested the bleeding from a stump, after amputation through the knee. I have also seen this method in the hands of my friend Mr. Banon, successfully restrain bleeding from a vessel deep under

the tongue, when he removed a large portion of the front of the lower maxilla. In the following case I tried it myself with happy results.

Thomas T., aged forty, a drummer in a militia regiment, was admitted into the Meath Hospital Feb. 20th, 1865. He stated that eight months previously he had received an injury of the left testis, by knocking it against the corner of a large chest. On the next day acute orchitis set in; the testicle soon suppurated; shortly afterwards the wall of the abscess became attached to the scrotum, which at last gave way, when a large fungoid growth protruded. On admission the tumour seemed to be that termed lipoma, and from its size the whole testicle appeared unravelled, and deranged. Its surface was irregular, of a dirty ash colour, and it exuded a thin fetid discharge. It gave the patient little or no pain; his health was not impaired, and there was no glandular enlargement in the groin or abdomen. Various astringent applications were used to restrain its growth, prior to the performance of Syme's operation of paring the edges of the scrotal aperture, pushing back the testicle, and stitching the edges of the scrotum over it. These appliances had no beneficial effect; and as he wished to be with his corps while under training, he insisted upon the more speedy cure by extirpation. Accordingly, on the 7th April, he being under the influence of chloroform, I performed the operation in the usual manner, by an incision commenced at the external abdominal ring, carried to the tumour, and then from the lower part of the growth, about half an inch towards the bottom of the scrotum; a longer incision not being required as the testicle protruded. Having then separated the testis from its bed, I isolated the spermatic cord, and passed under it a needle with a wire attached. Having then passed a wire loop over the point of the needle so placed, I brought it in front of the cord, and winding it under the other end of the needle (after Simpson's third plan), I tightly secured the entire cord between the needle and wire. I then divided the cord below the needle, and found the vessels perfectly restrained, without exuding a drop of blood. In addition to the arrest of bleeding, the needle, which lay across the top of the wound, prevented any retraction of the cord. The end of the cord was kept in view in such a way that if any bleeding had taken place the vessels could have been tied with the greatest ease. There was now only one other artery at the septum scroti to be secured. It was arrested by acupressure in the same way.

Having then shortened the needle with a nippers, I brought the wires out through the wound, and approximated the edges by three points of silver wire suture. April 9th.—I removed the needles with the greatest ease; the patient felt no pain, nor did a drop of blood follow. The wound healed kindly, a moderate amount of suppuration taking place. It was perfectly cicatrised on the 29th April, and the man was discharged from the hospital on the 5th May. From my limited experience of this method I could not venture to recommend the surgeon on all occasions to adopt this mode of sealing vessels, though nothing could be more satisfactory than its action in this case just related. One of the objections to acupressure is the pain produced by pressing on nerves; yet here the entire spermatic cord was enclosed between the needle and wire without causing much annoyance. The patient never complained of pain after he recovered from the anesthetic sleep. From my own experience, and that of some other surgeons, the great objection that might be urged to acupressure is that it is certainly more *tedious* than the ligature. But I have no doubt that we sometimes find bleeding vessels in situations where it will be found far easier to control the hemorrhage by this means than the ligature.

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ART. II.—*The Climate of Malaga, and its Influence on Chronic Pulmonary Diseases, especially Consumption.* By THOS. MORE MADDEN, M.D., M.R.I.A.; L.K. & Q.C.P.I.; M.R.C.S.E.; L.F.P. & S., Glasg.; Demonstrator of Anatomy in the Carmichael School of Medicine; author of *Change of Climate: a Guide for Travellers in Pursuit of Health, &c.*

THE following communication on the medical climatology of the city of Malaga, and its influence on health and disease, is founded on my experience of this climate during a considerable part of three winters; and in part, also, on long residence in various parts of the south of Europe and Northern Africa, which afforded opportunities of comparing the climate of Malaga with that of other localities resorted to in winter by British pulmonary invalids.

Since last year, when I published an account of this health resort in my work "*On Change of Climate*," I have been favoured by the observations of the most recent meteorologists in Malaga,



the results of which are embodied, with my own hitherto unpublished notes, in the present paper.

Before speaking of the climate of Malaga I shall, however, make a few remarks on some of the difficulties that embarrass the study of medical climatology, and some fallacies which I conceive prevail in this science, and then very briefly point out the climates which my experience leads me to believe are best adapted to the various forms of pulmonary disease.

The majority of writers on medical climatology are physicians established in practice in the southern *sanatoria* resorted to by invalids in pursuit of health, who, having themselves derived sanatory benefit from the climate in which they have settled, are, very naturally, disposed to regard that place from the most favourable point of view, and often, unconsciously, exaggerate its good qualities or fail to point out its defects. Moreover, many of these gentlemen do not appear familiar with any southern winter residence except that which they describe; and, although some of the local works on climate contain very lengthened, reliable, and valuable observations on the state of the weather and atmosphere, yet such observations by no means compensate for the want of those properly instituted meteorological comparisons which it requires an extensive personal knowledge of many climes to carry out effectually.

Mere tables seldom convey any accurate idea of the climate of a place; for, as a very able writer on the "English Exodus," in *The Medical Times and Gazette*, Oct. 15th, 1864, justly observes:—"Our sensations of heat and cold do not always tally with the rise and fall of the thermometer, nor our impressions of the lightness or heaviness of the atmosphere with the variations of the barometer; the sirocco may produce an intense feeling of lassitude and dejection, and yet the barometer hardly vary a line."

The point on which invalids going abroad really require information is the temperature of the winter and spring months in the place where they intend sojourning; but in works intended for such persons this point is generally passed over with a reference to some table of temperature, which conveys no distinct idea to the mind of the ordinary reader; therefore, in the following remarks I have endeavoured to compare the temperature, state of the weather, prevailing winds, &c., in Malaga with other known winter climates.

Fashion has a great influence on the reputation of the climates

frequented by English valetudinarians; thus Cannes owes its *renommée* as a winter residence mainly to the influence of a celebrated nobleman, who, some thirty years ago, passed a winter there, and, finding the climate suited him, has since returned each year, followed by others—so that the climate that suits a very distinguished person seems to possess some special attraction for English invalids. A medical man may, perhaps, attend in the wake, to look after the bodily infirmities of his exiled compatriots; and the doctor, having probably some time on his hands, writes a book in which he proves the superiority of the climate to his own entire satisfaction; and in some cases a very bad climate for phthysical invalids—as, for instance, Mentone—has thus been “written up” into temporary notoriety. Unfortunately laudations of unsuitable climates are seldom contradicted. Valetudinarians, having little the matter with them, visit Mentone and similar places, and, being benefited by travelling, and change of scene as well as of air, the result is vaunted as an incontestible proof of the sanative action of the climate; but we hear nothing of the cases in which invalids suffering from organic disease are injured by an unsuitable climate, and die in these localities. Thus it is that physicians at home are misled, and their patients abroad suffer the consequence.

Too much importance is attached to the mere temperature of a place in considering its eligibility as a residence for invalids. The influence of a climate on health and disease is the impression produced on the body by the state of caloricity, humidity, and electricity of the atmosphere—its pressure, purity, and amount of ozone it contains, all of which are modified by the topographical conditions of the locality. Now, the thermometer, so generally relied on as the test of climate, only indicates one of these conditions. It is obvious, however, that persons suffering from phthisis, chronic bronchitis, asthma, some periodic nervous affections, and, in short, almost all chronic diseases in which change of climate is resorted to, are highly sensible of, and are acted upon by all changes in the electrical and hygrometrical state of the atmosphere, its purity, the direction and force of the winds, and many other circumstances, none of which are in any way indicated by the thermometer.

When we have to choose between the various southern climates usually resorted to by British invalids in any case of pulmonary disease, the division of such climates into two great classes—*i.e.*, *tonic* or dry, and *sedative* or humid—becomes of most important practical assistance to the often embarrassed practitioner. And yet,

though four years have now elapsed since Dr. Corrigan urged the importance of this classification, in his admirable address to the Irish College of Physicians, to the present day it is not by any means sufficiently recognized in practice—people resting satisfied with removal to a climate so many degrees warmer than that of these islands, without taking into consideration the other equally important requisites of a good winter residence. In a valuable *brochure*, reprinted from *The Medical Mirror*, for May and June, 1864, “On Change of Air,” Dr. Thorowgood has also ably demonstrated the great utility of Dr. Corrigan’s simple division of southern climates.

The year before last a case fell under my notice which forcibly impressed me with the justice of Dr. Corrigan’s observations. In this instance the patient was a gentleman, aged thirty-seven, of a scrofulous diathesis, and phlegmatic temperament, suffering from the first stage of phthisis, marked by symptoms of great weakness and relaxation. In Malaga, where I had attended him during the preceding winter, he had derived so much benefit from that tonic, warm atmosphere that I thought it very probable that, by observing proper precautions, he might yet enjoy many years of comparatively healthy existence. Such was Mr. R.’s condition when I saw him in May. Five months subsequently, in December, I received a letter from him telling me that he was dying, and that ever since his arrival in Alexandria, where he had been sent by his medical adviser in England, his cough and dyspnea had become urgent, the expectoration had increased and become purulent, night sweats had set in, which, alternating with diarrhea, were rapidly exhausting his strength. I wrote at once, urging him to move up to Cairo; but it was too late, and shortly afterwards I heard of his death. Now, if a dry climate had been selected in this case, instead of an extremely humid one, the result would probably have been different; for, although Upper Egypt enjoys, perhaps, the driest atmosphere in the world, the air of the delta of the Nile, especially at Alexandria, is, particularly during certain winds, laden with vesicular moisture.<sup>a</sup>

<sup>a</sup> I find the following corroboration of this opinion in an ably written notice of the cholera now raging in Lower Egypt, published in the *Morning Star* London newspaper of the 1st of July, 1865, wherein a remarkable observation of a writer on Egypt, whose work was published thirty-four years ago, is thus cited:—“As an example of the exceeding humidity of the climate, we may quote a passage from Dr. R. R. Madden’s work on Egypt. ‘In Alexandria, when the air is excessively moist, I observed several mummies melt away in a dark magazine where I kept them, and decomposition took place after an exposure of forty hours to the humid atmosphere, though the same bodies had resisted corruption in a dry air for perhaps forty centuries.’ This fact is a most



The hygrometric state of the air, however, is not the only point on which errors are committed in the selection of climates for pulmonary invalids. When I was in Pau patients complained to me that hemoptysis had attacked them more severely, and at shorter intervals, after their arrival there than before they had left England. This result might have been anticipated had the practitioner who recommended that climate to a patient subject to pulmonary hemorrhage been acquainted with the effects which the thin, rarified atmosphere of a mountain district must produce on the capillaries of the lungs. I mention these instances only as a proof that climatology should form as essential a portion of medical study as any other branch of therapeutics.

Change of climate is resorted to in two varieties of phthisis, and is productive of equal service in both. As, however, the climate that would be most advantageous in one form of the malady would probably only hasten death in the other, I shall briefly point out the characteristics of both these forms of tubercular disease of the lungs, and the climates that are best suited for their cure or alleviation. A very considerable portion of the phthisical patients that are sent abroad from this country require a dry, warm, bracing, or tonic and equable atmosphere, being persons in a relaxed anemic condition, often of scrofulous diathesis, who suffer from profuse expectoration, and are weakened by night sweats or diarrhea. The principal *tonic* winter residences for these patients are Western Australia, Upper and Middle Egypt, Nice, Hyères, Cannes, Montpellier (during a part of the winter only), and Malaga. The second class of cases which are met with in the health resorts of Southern Europe and Africa are marked by frequent hard dry irritative cough, scanty expectoration, pulmonary hemorrhage, and fast irregular thready pulse. In such cases any of the climates I

remarkable one, and illustrates effectively the nature of this Alexandrian atmosphere. It is equalled by Dr. Barclay's assertion that during the time he was there he felt as if inhaling steam; his breathing was excessively affected, and his whole system was languid and relaxed. If we have thus endeavoured, at some length, to lay before the public the real hygienic condition of Alexandria, it is because we wish it to be understood that the presence of even a violent epidemic in that city is nothing extraordinary. IF REPORT BE TRUE, one hundred and eighty-four persons died of cholera on Sunday last in the hospital of Alexandria. The entire number of deaths recorded up to the present is between a thousand and fifteen hundred."

I may, however, observe that the writer of the article in the *Star*, to which I have referred, has forgotten to acknowledge the source of his information about the climates of Egypt, which he has done me the honour of taking, *almost verbatim*, from my work.

have just mentioned would prove too exciting. The locality selected for the winter residence of these patients should have a *sedative*, warm, and somewhat humid atmosphere; and the physician may choose between Madeira, Pisa, Rome, and in some instances Lisbon, according to the symptoms of each case. Algiers holds a kind of intermediate place between tonic and sedative climates, in some respects approximating to Upper Egypt and Malaga, while differing from them in others—the atmosphere being in many cases relaxing. It is occasionally very serviceable in both varieties of phthisis pulmonalis.

The division, generally adopted, of phthisis into three stages is of practical utility in the application of change of climate to the cure of the disease, each advance in the progress of the complaint modifying the selection of a suitable climate. I cannot, however, enter on this question here, but shall only say that, to be used with success, change of climate must be early had recourse to, and that nothing can be more cruel than to send a patient, advanced in the last stage of consumption, abroad, as a last resource, when the case has really become hopeless. I have, unfortunately, too often had to witness the deathbed of a consumptive patient who had been sent abroad when this measure, instead of being useful, must have been hurtful, and when death was probably hastened by the fatigue and excitement of the journey.

Facility of access is a point of great importance in the selection of a climate for invalids. It is obvious that a patient in an advanced stage of disease should be sent to a place which may be reached by an easy and agreeable journey, and that in case of need he may communicate quickly with his physician and friends at home. In this particular Malaga will bear comparison with any health resort on the Mediterranean, as it may be readily reached by either land or sea. Having more than once tried each route, I have no doubt of the advantages of the sea passage for invalids. The overland journey *viâ* Madrid and Granada should never be attempted by those in delicate health; the quickest route for such individuals is that *viâ* Paris and Marseilles by rail, and thence by steamer, touching at Barcelona, Valencia, and Alicante, to Malaga. There is also constant steam communication with the south of Spain from England, either by the Peninsular and Oriental Steamers to Gibraltar—only 80 miles to the east of Malaga—or directly to Malaga from Liverpool, and also from London. I should in general recommend the Liverpool route to consumptive patients. This

passage occupies about eight days, which is sufficiently long to afford the advantages of a short sea voyage. The equable temperature of the air at sea, its purity, the saline matter it contains, and the less variable pressure of the atmosphere, all co-operate, with the gentle unceasing motion of the ship, and the slight nausea generally experienced during the first few days on board, to bring the patient into a proper condition before he lands for receiving all the benefits derivable from the climate of Malaga.

The city of Malaga, one of the chief towns of Andalusia, is situated in a deep and beautiful bay, surrounded by a fertile valley opening to the south on the Mediterranean, and protected on the west, north, and east by the lofty mountains of Ronda, Antequera, and the Sierra Nevada. Some idea of the altitude of these ranges, and of the purity of the atmosphere, may be gathered from the fact, that from the Granada road, immediately behind the town, the view embraces the vast extent from Gibraltar, on the west, to Velez Malaga on the east, and across the Mediterranean to where the summits of the African Atlas are distinctly visible to the far south. I shall not here enter on any general description of the city of Malaga, but shall at once pass to what more directly interests medical inquirers, *i.e.*, the climate and topography of the place, and their influence on health and disease.

The latitude of Malaga should render its climate warmer than any of the health resorts of the south of France or Italy, as it lies 410 miles south of Pau, 486 south of Nice, 250 south of Naples, and upwards of 300 South of Rome. The climate of Malaga belongs to the class of climates described by Dr. Corrigan under the name of *tonic*. It is remarkably dry and equable, warm, and somewhat bracing. So steady is the temperature of the day that, according to my observations, the mean daily variation of the month of December was only  $2\frac{1}{2}^{\circ}$  (Fahr.); the greatest daily variation during that month was  $5^{\circ}$ , and the smallest was  $2^{\circ}$ . In January the mean daily variation was  $3^{\circ}$ ; the maximum  $9^{\circ}$ , and the minimum  $2^{\circ}$ . In February the average daily variation was  $2\frac{1}{2}^{\circ}$ ; the greatest  $7^{\circ}$ , and the smallest  $2^{\circ}$ . But, though the temperature of the *day* in Malaga is, perhaps, the most equable in Europe, yet the temperature of the twenty-four hours of day and night is not by any means so, for, immediately after sunset, there is a remarkably sudden and very perceptible diminution of temperature, frequently accompanied by a profuse fall of dew, rendering the atmosphere humid and chilly. It is, therefore, highly important to apprise invalids going



to Malaga of this fact, that they may avoid the great injury which results to those who adopt the common practice of strolling on the Alameda after sunset.

The mean annual temperature of Malaga is  $65^{\circ}$ , or  $16^{\circ}$  higher than Dublin,  $9^{\circ}$  higher than Pau,  $1^{\circ}$  lower than Algiers, and  $7^{\circ}$  lower than Cairo. The mean temperature of winter is  $55^{\circ}$ , or  $13^{\circ}$  higher than Dublin, also  $13^{\circ}$  higher than Pau, exactly the same as Algiers, and  $3^{\circ}$  lower than Cairo. In Spring the mean temperature is  $68^{\circ}$ , or  $24^{\circ}$  higher than Dublin,  $14^{\circ}$  higher than Pau,  $2^{\circ}$  higher than Algiers, and  $3^{\circ}$  lower than Cairo. The temperature of summer and autumn need not be considered, as Malaga is only resorted to by invalids during winter and spring. The last winter I spent in Malaga was a fair average season, and during the month of December the mean temperature of the day was  $59\frac{1}{2}^{\circ}$ ; the highest temperature I observed was  $68^{\circ}$ , and the lowest  $52^{\circ}$ . In January the mean daily temperature was  $57^{\circ}$ ; the highest  $61^{\circ}$ , and the lowest  $50^{\circ}$ . In February the mean temperature of the first fourteen days was  $58^{\circ}$ ; the highest  $66^{\circ}$ , and the lowest  $50^{\circ}$ .

Rainy days are less numerous in Malaga than in any part of Southern Europe frequented by invalids for change of climate. Thus, on an average of ten years, there are only 29 rainy days annually in Malaga, or 90 days less than in Pau, 88 less than Rome, 45 less than Nice, 44 less than Madeira, and 41 less than Algiers. During the year 1863 the number of days on which rain fell was greater than the average, amounting to 38.

The annual rain-fall is also comparatively small, being about  $16\frac{1}{2}$  inches. It is generally of tropical character, falling in large drops, and with considerable force, and is therefore of short continuance. The most rainy month is February; and during the three Februaries I passed in Malaga invalids were confined to their rooms for fully half the month by damp, and even cool weather.

I am indebted to Mr. John O'Kelly, of the Geological Survey of Ireland, for the following very valuable tables of his meteorological observations in Malaga, during the winter of 1863-64:—



*Meteorological Observations, Malaga, November, 1863.*

Day of Month	Attached thermometer 9 am.	Rain	WIND		Barometer reduced to 32° at sea level	Remarks
			Velocity	Direction		
1	..	..	..	..	..	On the 9th the thermometer exposed registered 114° in the sun, and 72° in the shade.
2	..	..	..	..	..	
3	..	..	..	..	..	
4	70	None	..	..	30.40	
5	70	"	..	..	30.30	
6	68	"	..	..	30.34	
7	68	"	..	..	30.14	
8	70	"	..	..	30.14	
9	70	"	Light	N.W.	29.90	
10	69	"	"	"	29.90	Exposed thermometer, in shade, 68°.
11	68	"	Fresh	"	30.00	
12	68	"	Rough	"	29.84	
13	66	"	"	"	29.84	
14	66	"	Light	S.W.	30.06	
15	65	"	"	"	30.15	
16	65	"	"	S.E.	30.18	
17	65	"	"	S.S.E.	30.12	
18	65	"	Fresh	E.S.E.	30.12	
19	66	"	Calm	—	30.24	
20	66	"	Fresh	S.E.	30.26	
21	66	"	"	E.S.E.	30.24	
22	66	"	"	E.	30.12	
23	66	"	Light	E.S.E.	30.00	
24	66	"	"	E.S.E.	30.00	
25	66	"	"	S.E.	30.06	
26	68	"	Fresh	"	30.10	
27	66	"	"	E.S.E.	30.02	
28	68	"	"	"	29.90	
29	68	Rain and storm in morning	Evening calm	E.S.E.	29.79	
30	68	None	Light	S.E.	29.92	

*Meteorological Observations, Malaga, December, 1863.*

Day of Month	Attached Thermometer	Exposed Thermometer in shade	Rain	WINDS		Barometer reduced to 39° at sea level	Remarks
				Velocity	Direction		
1	70	..	None	Light	E.S.E	30·02	Exposed thermometer at 10 p.m. 62° at 11 a.m.; 98° in sun.
2	69	..	"	"	W.N.W.	30·20	
3	70	..	"	"	N.N.W.	30·22	
4	71	..	"	"	S.E.	30·24	Barometer at 8 p.m., 30·38.
5	70	..	"	"	S.S.E.	30·32	
6	70	..	"	"	S.E.	30·34	
7	69	..	"	"	N.E.	30·26	
8	68	..	"	Calm	N.	30·25	
9	67	..	"	Light	E.S.E.	30·32	
10	64	..	"	"	"	30·34	Exposed thermometer at 9 a.m., 60°; at 6 p.m., 60°.
11	64	..	"	"	"	30·38	
12	68	60	"	"	S.S.E.	30·38	
13	68	60	"	Calm	S.	30·20	Exposed thermometer 60° at 6 p.m.
14	68	57	"	"	S.	30·22	
15	68	60	"	Light	S.E.	30·22	
16	68	60	..	"	Shifting, N. to S.	30·21	Cloudy in morning.
17	66	60	..	Fresh	N.W.	29·94	
18	64	56	Some rain, cloudy	"	N.W.	29·66	
19	64	54	..	Light	Shifting, N. to S.	29·82	
20	60	52	..	Calm	..	29·91	
21	58	52	Cloudy	"	..	30·02	
22	58	54	Bright	Nearly calm	S.W.	30·10	
23	62	54	"	"	N.W.	30·02	
24	65	58	"	"	..	30·18	
25	65	58	"	Calm	..	30·16	
26	65	56	"	..	..	30·14	
27	65	58	"	Fresh	N.W.	30·10	
28	65	58	"	"	Shifting, N.W. to S.E.	30·22	Wind N.W. in morning; shifted to E.S.E. in evening.
29	64	58	"	Light	S.E.	30·24	
30	65	58	"	"	S.S.E.	30·20	
31	65	..	"	Fresh	N.W.	30·02	Terral.

*Meteorological Observations, Malaga, January, 1864.*

Day of Month	Attached Thermometer	Exposed Thermometer in shade	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
				Velocity	Direction			
1	65	57	None	Rough	N.W.	30.02	..	Terral.
2	65	58	"	Fresh	W.	30.04	..	
3	65	60	"	Calm	"	29.96	..	Thermometer in sun, at noon, 88°.
4	62	52	"	Rough	E.	29.96	..	Stormy at night.
5	60	52	"	"	E.	30.02	Cloudy	Cold wind to-day.
6	59	52	"	Light	"	29.92	"	
7	59	52	"	Rough	S.E.	29.89	"	Stormy at night.
8	62	54	"	Fresh	S.E.	29.69	Bright	
9	62	..	Rain	Light	"	29.84	Cloudy	
10	62	..	Rain	"	S.	29.80	Cloudy	
11	64	..	None	Fresh	W.	30.04	Bright	
12	64	..	Rain at night	Nearly calm	S.	30.22	Morning fine	
13	62	..	Rain	"	S.E.	30.09	Cloudy	
14	63	..	None	Fresh	W.S.W.	30.12	Bright	
15	62	..	"	"	E.	30.21	Cloudy	
16	67	..	"	Light	E.	30.18	Bright	
17	65	..	"	"	E.S.E.	30.00	"	
18	65	..	Rain in morning	Fresh	E. to N.W.	30.00	Cloudy	
19	67	..	None	Light	E.	30.22	Bright	
20	63	..	"	Rough	E.	30.32	Cloudy	
21	62	..	"	Light	"	30.30	Cloudy	
22	63	..	"	"	"	30.30	Bright	
23	60	..	"	"	"	30.32	Cloudy	
24	64	..	"	"	"	30.30	Bright	
25	61	..	"	Fresh	"	30.22	Cloudy	
26	66	..	"	Light	E.S.E.	30.32	Bright	
27	61	..	"	"	E.	30.30	Cloudy	
28	64	..	"	"	E.N.E.	30.22	"	
29	66	..	"	Fresh	S.W.	39.02	Bright	
30	64	..	Rain in morning	"	"	30.02	Cloudy	
31	62	..	Some rain at night	Fresh	E.	30.12	Bright	

*Meteorological Observations, Malaga, February, 1864.*

Day of Month	Attached Thermometer	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
			Velocity	Direction			
1	64	Rain in morning	Generally fresh	E.S.E.	30.12	Bright	No rain fell from 8 a.m.
2	62	None	"	"	30.30	"	
3	62	"	Light	S.E.	30.32	"	
4	64	"	"	E.S.E.	30.20	"	
5	64	"	Fresh	E.N.E.	30.00	"	Sharp wind, snow on hills.
6	60	"	"	E. to N.	29.94	"	Ditto
7	60	"	"	"	30.02	"	Ditto
8	60	Rain in evening	"	N.E.	30.00	Cloudy	Ditto
9	60	Rain in morning	Light	"	29.64	"	
10	62	None	Fresh	N.E. to S.E.	29.90	Bright	
11	62	"	"	N.	29.84	"	
12	66	"	Light	S.E.	30.60	"	
13	66	"	"	S.W.	30.24	"	
14	69	"	"	"	30.32	"	
15	69	"	"	S.S.W.	30.22	"	
16	65	"	"	N.W.	30.20	"	
17	64	"	"	N.W.	30.04	"	
18	64	"	"	"	29.74	"	
19	65	"	Fresh	"	29.54	"	Barometer, 7 p.m., 29.42.
20	66	"	"	E.	29.42	"	
21	65	"	"	E.	29.50	"	
22	60	Rain	Rough	E.	29.60	Clouds	
23	60	"	"	E.S.E.	29.72	"	
24	60	"	Light	"	29.80	"	
25	64	"	"	"	29.84	"	
26	63	Showers	"	"	29.90	"	Exposed thermometer, at 12 o'clock, 71°.
27	64	"	Calm	"	29.78	"	
28	63	Rain	"	"	29.70	"	
29	64	Fine	Light	S.W.	30.01	Bright, some clouds	



*Meteorological Observations, Malaga, March, 1864.*

Day of Month	Attached Thermometer	Rain	WINDS		Barometer reduced to 32° at sea level	Sky	Remarks
			Velocity	Direction			
1	64	None	Light	S.E.	30.00	Bright	
2	63	"	Calm	E.S.E.	29.98	"	Very fine.
3	63	"	Light	"	29.00	"	
4	65	"	Calm	"	29.66	"	
5	65	Rain	Rough	S.W.	29.40	Clouds	Very wet; barometer, 10 p.m., 29.86.
6	66	None	Light	"	30.00	Bright	Very fine.
7	66	"	Fresh	S.S.E.	29.99	"	
8	68	Showers	"	S.E.	29.80	Some clouds	
9	68	Rain in morning	..	"	29.74	Clouds	

The mountains beyond the Vega almost completely shelter the town from the north and west winds. The prevailing winds are the east, or *levante* which is cold and humid in winter, the north-west and the south-east. The following table shows the

*Frequency of the various Winds during the year 1863.*

Winds	Number of times they occurred	Remarks
North to east, .	140	} These observations were taken three times a day.
East to south, .	460	
South to west, .	143	
West to north, .	352	

The north-west wind is termed *un viento fatal*, a deadly wind, by the natives. It rushes through a gap in the Antequera mountains, called the *Boca del asno*, along the valley of the Guadalmedina, and arrives in Malaga laden with fine sand, which irritates the pulmonary mucous membrane. In summer this wind is hot and dry, giving rise to a highly irritable and nervous state of mind and body. It is a curious illustration of this, that the cases of stabbing, for which Malaga is unfortunately so notorious, bear each year a constant proportion to the number of days the *terral* has prevailed. In winter the north-west wind is still a dry wind, but, sweeping down the snowy mountains, is then intensely cold. Its ill effect,

however, cannot be measured by the thermometer alone; for the force and rapidity of its motion, its aridity, and the quantity of impalpable sand it suspends, all combine with its low temperature to injure the valetudinarian.

We must also bear in mind that the *terral* suddenly takes the place of the warm and genial air that usually prevails in Malaga; and thus produces all the ill effects of a great, rapid, and unprovided-against change of climate. While the *terral* lasts, the hands and lips become chapped, a tendency to epistaxis is produced, cough and expectoration are increased, and changed for the worse; and, in consumptive cases, hemoptysis is frequently brought on. The general health, too, suffers, and, the mind sympathising with the body, the patient becomes irritable and despondent. It is, therefore, essential to impress on the patients we send to Malaga the necessity of remaining within doors as long as this wind prevails, which, fortunately, is seldom for more than three consecutive days. This advice is the more important as all foreign invalids in Malaga reside on the Alameda, or public promenade, which is fully exposed to the *terral*.

The last wind I need speak of is the South-east, which in its passage from the opposite African coast, over eighty miles of sea, absorbs a good deal of moisture, and in winter is regarded as mild and healthy. In summer, however, it resembles the Sirocco (the *plumbeus Auster*) of the Italian coast in its effects, and occasions the well-known feelings of malaise and depression which that wind produces.

According to Senor Montes, the barometer is very steady in Malaga; having taken no notes on the point I am unable either to corroborate or dispute this statement. I am, however, indebted to Mr. O'Kelly, of Rochestown, for the following table, kept by a resident observer, showing—

*The State of the Barometer during the year 1863.*

Mean height of barometer at 9 a.m.,	.	.	29° 1.7"
" " " 3 p.m.,	.	.	29° 1.
" " " 11 p.m.,	.	.	29° 1.4"
" " " during the year,	.	.	29° 1.4"
Maximum,	.	.	29° 8.5"
Minimum,	.	.	28° 2.9"

Snow falls very rarely in Malaga; so seldom, indeed, that when

Senor Martinez published his *Topografica Medica de la Cuidad de Malaga*, in 1852, he states (p. 145) that there was no record of its ever having snowed within the city. But since then a small quantity of semi-fluid snow fell one day in February, 1860, to the great astonishment of the Malaguenans. It must be borne in mind, however, that although cold weather occurs but seldom in Malaga, and lasts for only a very short time, yet it is far more severely felt there than it is in this country. For in Malaga the houses are built with the intention of excluding the certain and intense heat of summer, and no precautions are taken against the less certain cold of winter. In the hotels and lodging-houses the rooms are large and sparingly furnished, there are neither carpets nor window curtains; the only means of warming the apartments is a *brasero*, or pan of charcoal, which is a poor substitute for the cheerful blaze of an open fire; and as the smouldering charcoal, of course, evolves carbonic acid gas, is dangerous as well as unpleasant.

I believe that the benefit which results from sending our consumptive patients to southern climates is in a great measure owing to the fact that in such climates invalids may pass most of their time in the open air, and take far more out-of-door exercise than they could in this country. For example, in 1863, the following was the state of the weather and atmosphere at Malaga—

Fine bright days, . . . . .	213
Cloudy, . . . . .	75
Foggy, . . . . .	34
Days on which some rain fell, . . . . .	27
Wet days, . . . . .	11
Stormy, . . . . .	5

While in Dublin, at the same time, there were 176 days on which the sky was overcast, and 212 days on which rain or snow fell.

With respect to the living in Malaga, which is a very important point, as it is essential that the little sustenance an invalid takes should be nutritious and digestible; it must be admitted that in both respects the animal food in Malaga is very inferior to that used in this country. The meat there is generally hard, stringy, lean, and flavourless, except the pork, which, as throughout Spain, is excellent, but is not a fit food for invalids. The fish, however, is of such good quality, and of so many various kinds, that it makes

up in a great measure for the shortcomings of the animal food, and invalids may manage to live very well and very cheaply at the hotels, on fish, kid, turkeys, ducks, fowl, game, especially partridge, which is a standing dish here, and vegetables and fruits, many of which are unknown, except as rarities in this country, even if they do not choose to venture on the meats served at the table d'hôte.

Spanish cookery is generally considered as intolerable by British travellers, but I think this is mere prejudice; and that garlic and oil, which enter so largely into all culinary operations in Spain, are absolutely necessary (moderately used) to supply the want of fat and of flavour in the meat, and to render it more digestible.

The physician who sends his patient to Malaga should impress on him the great importance of selecting an apartment having a southern aspect, as there is often a difference of ten degrees in the temperature of rooms facing the Alameda and those at the back of the hotels. Besides the mere warmth, the front rooms are more cheerful, and enjoy the advantage of free exposure to the light and sun, a very essential matter for a pulmonary invalid, who should recollect the Italian proverb, that—*Dove il sole non entra il medico viene*, “where the sun does not enter, the doctor must.”

The hygienic condition of Malaga is as defective as it can well be. In a great many of the houses there is no provision for sewerage of any kind; and even in the more civilized part of the city, in the hotels on the Alameda, the drainage is very bad indeed. The main sewers, which run under the principal street, are choked up by the decomposing accumulation of years, and being provided with immense square openings, through which the dirt and rubbish is thrown into them, in the centre of the streets, the mephitic gases evolved below freely escape into the atmosphere of the narrow lanes of the city. The bed of the Guadalmedina is really the main sewer of Malaga; and as for nearly ten months annually it is little more than a wide dry bed of gravel, being dependant on the torrents in winter for its purification, the odour it exhales in warm weather renders a residence near it as disagreeable as it is unhealthy.

The connexion between epidemic disease and bad sewerage is, I think, very well illustrated in Malaga, which has at all times been remarkable for the prevalence of zymotic diseases. I have collected from the older Spanish writers notices of no less than twenty-two epidemic pestilences, some of which almost depopulated the city between 1493 and 1804. The earlier of these seem to have



been epidemics of genuine oriental plague, and the latter generally assumed the form of yellow fever. Of late years, since 1834, these pestilences have not appeared, but their place has been taken by Asiatic cholera, which has several times ravaged the town.

From the returns of the civil hospital of Malaga we learn that the most unhealthy month there is January, and that, strange as it seems, less sickness occurs in December than at any other time. The most prevalent diseases are the eruptive fevers, and especially variola; intermittent and bilious fevers, acute affections of the respiratory organs, such as catarrh, bronchitis, pleurisy, and pneumonia, the latter being often conjoined. Acute and chronic maladies of the gastro-intestinal organs also prevail extensively. Diseases of the eyes are very common here, and I know of no country, except Egypt, in which one meets with more blind people. That rare and interesting malady elephantiasis arabum is occasionally witnessed in this town; and during one year no less than seven cases of it were admitted into the civil hospital. Some of these cases which I saw in that institution were as well marked instances of elephantiasis as I ever met with in any part of Africa I have visited.

In my work on change of climate, published in 1864, was given, I believe, the first account of a peculiar affection which frequently attacks strangers within a few weeks of their arrival in Malaga. It consists of a very painful acute inflammation of the margin of the gums, leading to the formation of minute ulcers round the teeth, and attended with considerable constitutional irritation. The natives attribute it to some article of the diet, but I think we may ascribe it to the Catalan wine ordinarily used in the hotels. This wine which is strong and acrid, and slightly acid, leaves a considerable deposit of tartar in the vessels in which it is kept even for a few days—and I think it is not improbable may act as an irritant on the mucous membrane of the gums. The treatment consists in stimulants and astringent gargles; low diet, and cooling laxatives are required, and I have sometimes been obliged, after the failure of other means, to employ freely a strong solution of nitrate of silver to the ulcers, notwithstanding its disagreeable effect on the teeth.

Senor Martinez Y. Montes, Physician-in-Chief to the Military Hospital, has collected tables from which we find that in January the number of deaths was larger than in any other month, while in May, it was smaller. The total number of adult deaths during

the year was 9,049. The greatest mortality from any one disease was 711 from dropsy; and the smallest was one death from hydrophobia. Acute and chronic diseases of the respiratory organs, not including consumption, were the cause of 1,208 deaths. The mortality from phthisis was 407, more than half of which, or 234, occurred in the civil hospital. Cerebral affections are apparently a prolific source of mortality here; 742 fatal cases of these complaints occurred, of which no less than 407 are set down to apoplexy.

In Malaga the deaths occasioned by consumption, among the native population, are less numerous than in any other European southern locality resorted to by invalids in winter. This is, I think, a very significant fact; for, as we might naturally consider a place where the mortality from phthisis was very great an unsuitable residence for phthisical invalids, so, a city like Malaga, where the mortality from that disease is remarkably small, should be a favourable locality for such patients. Its superiority in this respect to our own climate is sufficiently proved by the fact, that in Ireland 125 deaths out of every thousand are caused by consumption, while in Malaga only 34 deaths per thousand result from that disease.

It is a curious circumstance, however, that the mortality from phthisis in Malaga is becoming notably greater each year; and though, of course, some part of this increase is owing to the growth of the population, and a still larger proportion is occasioned by the deaths of foreign invalids, yet a considerable number of phthisical deaths remain which cannot be thus accounted for. Hence a belief in the infectious nature of consumption is very generally entertained here, and many of the lodging-house proprietors object to receive phthisical invalids. I may here observe that the belief in the communicable character of consumption prevails very extensively in Southern Europe, especially in the south of Spain and Italy, and particularly in Naples. This opinion, though it may be traced back to many of the old writers, will now find few supporters in this country. I cannot help thinking, however, that, in hot climates at least, there is a good deal of truth in it; and that constant communication with consumptive patients, such as sleeping in the same room with them, and breathing an atmosphere charged with the morbid exhalations from their diseased lungs is very likely to prove injurious to persons in delicate health, and may even determine the occurrence of phthisis in individuals who might otherwise have escaped this malady.

When we come to consider the mortality from all chronic diseases of the air passages, we find the superiority of the climate we are discussing is not so conspicuous as it might at first appear; for the deaths in Malaga from all chronic affections of the respiratory organs are very nearly as great as in Dublin, amounting, according to Senor Martinez Y. Montes, to nearly one-ninth the entire number recorded.

The dry, warm climate of Malaga is occasionally of great service to children hereditarily predisposed to tubercular diseases; and also to young persons in whose constitution a scrofulous taint is lurking, which if it does not affect the glandular system, may, about the age of puberty, pass into the premonitory symptoms of consumption.

In cases of tuberculous cachexia, and in that state of pulmonary irritation which always precedes the deposit of tubercle in the lungs, sometimes described as the "pretubercular stage of phthisis," the climate of Malaga produces its most decided curative result. Such cases may often be permanently cured by a sufficiently protracted winter residence in this climate for several successive years; during which the same precautions must be observed, and the same regimen and medicinal treatment be persevered in which would have been necessary had the case been treated at home. When the first stage of the disease has developed itself, and when a small amount of tubercular deposit has taken place, the case, though less under the influence of the climate, is still so to a great extent, and the foregoing observations are applicable to it. When the malady has advanced a step further, and some softening of the tubercles has occurred, then the chance of perfect recovery is of course greatly lessened; but the progress of the disease may be still arrested, and may be so long staved off that the patient may ultimately die from some other cause. To accomplish this end he must, however, live constantly under medical rule and in a suitable climate; and there is probably no European climate more proper for the purpose than that of Malaga.

It has been asserted by a very eminent authority that when a cavity exists in the lungs—"a southern climate is not only useless but injurious." I have, however, seen patients who when they landed in Malaga could hardly walk from the jetty to their hotel on the Alameda, so weakened were they by hemoptosis, increasing cough, night sweats, and purulent expectoration, and who presented the physical signs, as well as the symptoms, of a cavity, and yet in a few weeks were so improved that they could ride some miles to

a pic-nic, enjoy themselves there, and return home, apparently none the worse for the fatigue and the excitement. When the symptoms were so mitigated the patient would generally imagine that his disease had yielded to the climate, and would, with the characteristic hopefulness of the complaint, indulge in distant expectations and plans for the future. But, in most cases indeed, the improvement was only temporary, the symptoms slowly returned, and the fatal event was not prevented, but only postponed. Any prolongation of life, however, is considered so great a boon that a measure like this which affords a hope of adding to a life which must be shortened by consumption if unchecked, and even offers a possible chance, however small, of curing the disease itself, must be considered as an important addition to our mode of treatment. I think that very few phthisical patients are sent to Malaga who do not require cod-liver oil; but unfortunately many invalids as soon as they arrive there think that the climate alone should effect the cure, and lay aside the remedies they had been taking at home with advantage. Patients should, therefore, be warned against such an error; and, as the medicines sold in Malaga are seldom good, they should bring a small stock of this oil, a little tannin, and whatever else the physician considers necessary along with them. Cod-liver oil is, I believe, even more necessary in Malaga, where the meat is dry, lean, and innutritious, than in this country where it is the contrary.

Asthmatic patients often derive singular benefit from the climate of Malaga. But this climate cannot of course be applied indiscriminately to all cases, for it is obvious that an atmosphere which would serve a case of dry asthma must be injurious to the humoral form of the complaint. For instance, the moist warm air of Pisa, which would probably give marked relief in a case of spasmodic nervous asthma, would only increase the sufferings of an invalid labouring under the other form of the disease.

Chronic bronchitis is another malady on which change of climate exercises a very sensible influence, and it is often surprising how slight a change of air will cure chronic bronchitis that has resisted all the ordinary remedies. Within my own experience I have six or seven times seen a change from Dublin to the south side of Dalkey or Killiney, not ten miles distant, followed by immediate relief of a harassing bronchitic cough, which had been previously treated in the usual manner without benefit. But in more confirmed cases, the change of climate must be great and complete to do good, and a winter in Malaga sometimes agrees admirably with



elderly patients suffering from this complaint. In some instances, however, of a more irritative form of the disease, I have seen a fresh attack of acute bronchitis brought on by this climate, and one case that I attended resulted fatally from that cause.

It is quite as important to know what patients should avoid any climate as to know what class should select it. Now, considering the great mortality from cerebral affections, and particularly apoplexy, in Malaga, I think that patients predisposed to such diseases should not choose this town for their residence. Nor would I send a patient suffering from chronic rheumatism, or rheumatic arthritis, or neuralgia, to Malaga; as the great difference between the temperature of the day and night, and the heavy dews that fall after sunset, render this town in such cases inferior to other climates, such, for instance, as Western Australia, Upper Egypt, and in some cases Nice. Nor can Malaga be advised to dyspeptic and hypochondriacal invalids as the dietary there is not generally suitable for these cases.

The climate of Malaga was considered by the older native writers, Don Fernandez Barea, Padre Garcia de la Lena, and others, to exert a relaxing and unfavourable influence on the constitution of young persons under the age of puberty. But they regarded this locality as an advantageous residence for the old, and thought the climate a propitious one in which to "husband out life's taper at its close," and calculated, by aiding in the alleviation of the many physical annoyances of the aged, to add to the span of their existence.

In conclusion, I have only to express my opinion, that when a very dry, warm, tonic winter climate is indicated in the treatment of chronic pulmonary disease, especially phthisis, Malaga will be found superior to any health resort in Europe, and only inferior to Western Australia and Upper Egypt.

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ART. III.—*Remarks on Hydrocephalus; its Diagnosis, Prognosis, and Treatment.* By HENRY KENNEDY, A.B., M.B.; Fellow and Censor of the King and Queen's College of Physicians; one of the Physicians of the Cork-street Hospital.

[Read at the Dublin Obstetrical Society, 11th February, 1865.]

TWICE already the attention of this society was directed to the subject of hydrocephalus. On the first occasion I spoke of a form of the disease which was peculiar in this respect—that it occurred

at a more advanced period of life than the more common disease, being met between eighteen and twenty-five years of age—that it was a disease of a very insidious character, and almost necessarily fatal. As the paper is in print, more need not be said about it here. In the second paper some cases of the disease, as met in childhood, were detailed, in which recovery took place under very unpromising circumstances. It is this last phase of the subject I would again bring under notice; for it must be allowed that hydrocephalus is a disease entailing a great mortality. I recollect perfectly well, some twenty years ago, regarding the disease as all but hopeless. I now believe that, though it must still be regarded as a very serious affection, it may yet be looked on in a much more favourable aspect, inasmuch as recoveries are not uncommon, and, what is of more consequence, are directly due to treatment.

Of the nature of this affection there would seem now to be little doubt; and all writers are, I believe, at present agreed, that it is essentially found in the strumous constitution. Cases may, and I know do, occur where the evidences of such a state are wanting; but these are very exceptional, and only prove the rule. Nor is it a trivial matter to have a correct idea of the kind of constitution in which hydrocephalus occurs, for on this turns—necessarily turns—a great deal relating directly to the diagnosis, prognosis, and treatment of the disease. It has been my lot to examine more than twenty cases after death, and in all, besides the mischief in the brain, other organs, as the lungs, liver, and spleen, were likewise diseased more or less by the deposition of tubercle, as it is usually met in childhood. With such evidence I never meet a case of hydrocephalus now that I do not consider and treat as if it were a strumous constitution with which I had to deal. I have stated that the prognosis is modified by the views we take of the nature of this disease, and probably a few remarks on it, as well as the diagnosis, will not be considered out of place.

The diagnosis of this disease, it will be allowed, is often sufficiently puzzling. I do not speak of it when it is fully formed, and it is scarcely possible to mistake it, but at an earlier stage, when it is of every consequence to recognize it. I presume I address few who have not met cases of worm, or gastric fever, as it is called, which have suddenly changed their character—symptoms of water on the brain coming on, and so ending life. The same remark, too, may be made of the other fevers of childhood, such as scarlatina, measles, and hooping-cough, if the latter be a fever—in each and

all of which I have seen the disease supervene, and thus bring to a fatal termination cases which had begun with comparatively mild symptoms. Some of these cases have happened to myself, and more suddenly than was at all desirable, and I know they have occurred to others. Hence the greater need of caution, and being ever on our guard; and were I asked whether these sudden changes occurred in cases commencing with mild or heavy fever, I would at once answer the former.

Now the question may here be asked—why is it that such cases occur, or seem to occur, suddenly, and have we any means of foreseeing them? My answer is, that hydrocephalus is just like other affections in this respect, that its symptoms vary, and some of the more striking of them may be entirely absent, or so little marked as to escape notice. The impression with some is that every case must have vomiting; but, general though this sign be, it is not by any means constant; and anyone familiar with the morbid anatomy found in the brain in these cases, will see at once why it is not always present, for the same parts of the organ are not constantly engaged, nor the origin of the eighth pair necessarily involved. Hence it is that, just as may occur in pneumonia or pericarditis, hydrocephalus may likewise be more or less latent as regards its symptoms, and so may steal a march on us.

In helping us to a correct diagnosis—and I am, of course, still speaking of cases where the disease is obscure—it has appeared to me that much may be gained by learning what goes on during the night. All diseases are liable to exacerbations at this period of the day; but, in addition, we know that sleep is induced, in part at least, by an altered state of the circulation in the brain as contrasted with the waking hours. Hence a disease like hydrocephalus is very apt indeed to be attended by symptoms which show themselves, in the first instance, at night. Whether the explanation be correct or not, certain it is that a knowledge of what goes on during these twelve hours will often materially aid us in arriving at a sure diagnosis, and, if I may so say, foreseeing what is about to occur—a matter of no little moment to us all.

Into the general symptoms of the disease it would not be possible here to enter. They have been ably described by many, though possibly by none more so than by the late Dr. Cheyne, whose admirable work must even yet be considered a standard one on this disease. There are, however, two or three points worthy of a passing remark. Thus, in the class of cases of which I already

spoke, where the disease seemed to commence as gastric or worm fever, I have had occasion often to observe, when head symptoms threatened, a marked change to occur in its type. The pulse has become, in a marked degree, stronger, the skin hotter, and the tongue still whiter. In fact the fever has become much more of the inflammatory type; and this, let it be observed, before any complaint was made of the head. So that such an event occurring should at once put us on our guard.

But again: whilst we know that pain in the head is a very constant symptom of the disease, it is not always present, and I have seen several cases where the nape of the neck was the part of which the child first complained—cases, I mean, which ultimately ended in water on the brain. This is a point of some consequence, as it shows the part of the brain about being attacked, that is, close to the pons and upper portion of the spinal marrow; and besides, I believe our treatment cannot be applied too close to the seat of mischief. It is in these cases, when pain is referred to the neck, that we so often observe the child lying with the head drawn back, and more or less fixed—a state which, I am sure, all present must have witnessed.

Before leaving the subject of the diagnosis of hydrocephalus, I would notice a state, which if not the disease itself, is so very like it as to be worthy of remark here. It is a state with which I have been long familiar; but my recent connexion with the Cork-street Hospital has brought it much more prominently before me. What I allude to is a mixture of typhus fever, as shown in most of them by the presence of spots, with very marked brain symptoms. At certain periods there has been a run of cases of this kind which is quite remarkable. A sketch of two or three of them will illustrate the disease better than any mere description.

CASE. I.—Fitzgerald, aged seven years, admitted in March, 1853, with the heaviest fever a child could exhibit. The tongue was dry as a board, and the lips covered with sordes. With this state he had every symptom of water on the brain, except vomiting. He had dilated pupils—one more than the other; sighing; grinding of the teeth; squinting. Finally, convulsions and death.

CASE II.—In July, 1855, a boy of ten years of age was sent into Sir P. Dun's Hospital by Dr. Macready. When I first saw him he had very heavy fever, and was spotted. He was quite



stupid, and hard to rouse; had a marked frown on his forehead, and any complaint he made was of his head. His pupils were both dilated; pulse rapid; discharges from the bowels blackish; and every now and then he had the cry so characteristic of hydrocephalus. But he had no vomiting, nor grinding of the teeth. For my present purpose it is enough to say this boy recovered, but very slowly—the pupils gradually coming round to their normal state.

Case III.—Coleman, aged ten years, admitted to hospital in November, 1855. He had the regular typhus, and was spotted. His tongue was too clean; whilst there was oozing of blood from his lips and gums. Two days later signs of the brain becoming engaged appeared. He had severe pain in the head, and marked screaming; whilst the pulse, previously rapid, fell to 60. From this state he very slowly emerged. There was not, either, in this instance, any vomiting. Other cases very similar to these could be given, but what have been detailed are quite sufficient for my present purpose. They show that in the progress of spotted fever brain symptoms having a very close resemblance, I do not say identity, with hydrocephalus may arise. They differ, however, from the more regular disease in not being at all so fatal; for, though they are always serious, the majority recover, which cannot be said of the other form of brain affection. It has appeared to me that the typhus poison, which was unquestionably present, as shown by the spots, caused such a revolution in the system as to overcome the tendency to engender hydrocephalus, for that there was such a tendency appears to my mind to admit of no question. But inviting though this part of my subject is, I must hasten on.

And now, supposing the diagnosis of hydrocephalus made, what is to be the prognosis? Must it necessarily be fatal? or are there any reasons why, in some instances at least, better hopes may exist? I believe there are; and for my own part cannot doubt that I have now seen several cases, where the disease was well marked, recover, and before concluding this paper shall detail some of them. If, then, there be such cases, and for the present I shall assume there are, it will be well we should be able to recognize them, as on such knowledge it is our prognosis must obviously be founded. The points, then, that appear to me of every consequence in forming our judgment are mainly two. The first, and by far the most important, is the previous history of the child, for the longer the

attack has been coming on the more serious will be the result—I mean before the brain symptoms declare themselves. It will be recollected from what has preceded that we must consider the disease as essentially a constitutional one; and though I cannot state it as a positive fact from *post mortem* examination, I have got a strong conviction that in many cases mischief elsewhere has preceded the appearance of the cerebral affection. In other words, that the latter is but the finale of a state of disease which had been going on long before the brain became engaged. In this view of the matter it will be understood how important it is to get a thorough knowledge of the antecedents of the case, and every point, no matter how trivial, should be most minutely investigated. For myself, I believe that this knowledge is of very much more consequence in forming a correct prognosis than any present symptoms which the patient might present. As, however, it is only the general principle I speak of here, it will not be possible to enter into particulars, further than to say that loss of flesh and appetite, wandering pains through the body, derangement of the bowels, and, above all, the slightest evidence of cough, are amongst the most important symptoms for which we should enquire. Even in hospital cases, where it frequently happens we can get no satisfactory account of the previous state of the patient, a single glance reveals much. It tells us the shape of the chest, and general conformation of the body, besides all those marks which need not here be detailed, but which betoken the strumous diathesis, for it cannot be too strongly impressed on our minds that this it is which makes hydrocephalus such a formidable disease. As a corollary, then, from what has been just stated, it may, I believe, be taken for granted that if we meet the disease in a child otherwise healthy, and with none of those precursory symptoms at which I have glanced, we may have more hopes of the final result, and modify our prognosis accordingly.

In the last place, I would make a few remarks on the treatment of this disease. This, however, will be best done after giving the briefest detail of a few cases, each of which presented some feature of interest. The following is the only case I have seen of what may be called acute arachnitis of the brain occurring in childhood, and I introduce it here as affording a marked contrast to the cases which follow it, and as bearing out some of my previous remarks. I saw the case many years back with my friend, Dr. Faussett, now of Clontarf:—

CASE IV.—A very fine-made boy, of four years of age, was reported to have got a severe fall, a few days subsequent to which he began to complain of pain in his head. This increased rapidly, and was shortly followed by very severe vomiting. He had been dull and listless for a day or two before he made any complaint of his head. The very active treatment used had but a temporary effect in checking the symptoms, and at the end of the second day from the time the vomiting set in, convulsions came on, and the boy died on the fourth day. Examination disclosed a quantity of pure pus, chiefly on the anterior lobes of the brain, rather more being on one side than the other. A number of bloody clots were observed on cutting into the substance of the organ. Some turbid fluid was found in the ventricles.

CASE V.—A boy of eleven years of age was admitted into hospital, after, as it was stated, being a week ill. He had not been treated in any way, and presented all the signs of hydrocephalus in the second stage—his pupils being much dilated, the left the most, and his pulse being only sixty-four. The case passed into the third stage, and the boy ultimately died. But I mention it here because it was the first case I had seen where there was even a hope of life; for, though he died, it was after a very prolonged struggle. This case occurred as far back as the year 1841.

CASE VI.—A boy of nine years of age got symptoms of fever, and very soon began to complain of his head, which he could not bear to be stirred. His eyes were much suffused, and the light caused him great distress. Though he had vomited, it was not a prominent symptom. It was clear the brain was seriously threatened; and the case was the more serious as two children in the same family had already died of water on the brain. With this knowledge I did not hesitate to treat the case very actively. Leeches were applied three times within the first thirty hours, and other measures equally active employed. Under these means the head symptoms steadily yielded, and I had the pleasure of being able to pronounce the boy safe by the eleventh day. His ultimate recovery, however, was singularly protracted, and at this period I had the assistance of the late Sir Philip Crampton. This case occurred in 1844, and I think it right to state that in a similar case now I would use a very much modified treatment; for, though the boy recovered, he showed signs of great delicacy for a long time afterwards. He is now however, a vigorous man, close to thirty years of age.

CASE VII.—In the year 1849, and at a time when there was a regular run of cases of hydrocephalus, a girl, aged six years, was admitted to hospital with all the signs of the disease, in the second stage, well marked. She had very marked rigidity of the neck, and lay with the head drawn back; the pupils were dilated, one more than the other, and the peculiar cry of the disease was only too plainly heard. The pulse at this stage was slow, and she had several fits affecting one side of the body. I confess I looked on the case as hopeless. What was my surprise to find that, after ten days continuance of these symptoms, signs of amendment gradually, very gradually, began to appear. The intelligence improved, the screaming and rigidity slowly disappeared, and the child finally recovered.

CASE VIII.—Hamilton, a boy of six years of age, when first seen in hospital, had every sign of water on the brain. He was very stupid, and had the constant cry of the disease. It is unnecessary to give more particulars, except that he also recovered.

The next case, though ultimately fatal, has too close a connexion with my subject to be omitted here:—

CASE IX.—In January, 1855, Robinson, a girl of twelve years of age, was admitted with all the signs of water on the brain. The fever was of a more intense character than is usually present in such cases, but she made constant complaint of her head, whilst her pupils were dilated to the utmost, and the eyes had the look of blindness which all who have seen this disease must have witnessed; there was also vomiting, and she passed under her; the cry, too, was present. Yet from this state the girl recovered. As the brain symptoms, however, lessened, mischief began to declare itself in both the lungs and abdomen; and, in spite of every care, she ultimately sunk at the end of some weeks; I cannot doubt that strumous disease existed in both cavities. This was a remarkable case, and the only one I have seen where the disease ran this particular course. The patient, I should state, had entirely got free from the head symptoms.

Though I could give other cases, I shall limit myself to one more, very recently in hospital, and seen by Dr. Grimshaw and Dr. Barnes, now in Dublin:—

CASE X.—Harford, a girl of twelve years of age, was admitted



into the Cork-street Hospital on the 21st of December last. She was tall for her years, and thin, and laboured then under a severe attack of typhus fever, being profusely spotted; the brain was much engaged; she was very restless, and rambled constantly, disturbing the ward by her cries. This state continued longer than usual, but finally subsided, and all the signs of typhus had passed away—so much so, that she asked to be allowed up. At this period she began to complain of pain in her head, referred to the forehead, and with this I found that considerable fever had lighted up; the pulse was quick, the skin hot, and tongue furred. From this on every symptom of water on the brain developed itself. The pulse went through the regular stages of the disease, even to the third; the pupils became dilated; the girl quite stupid, and apparently stone deaf, and the peculiar cry was constant; the arms were rigid, though not in a marked degree, but there was no convulsion. Though she swallowed badly, she did not at any time lose the power entirely. From this state, which lasted about nine days, she gradually emerged, and very interesting it was to observe the daily progress. The dilatation of the pupils was the last symptom to yield; and even when she left hospital, a week since, they gave me the idea of being still sluggish. From the commencement of the brain attack till she left was just five weeks. This was a case of great interest, growing up, if I may so say, under our very eyes, and reaching a stage of the disease from which few indeed recover. Still I trust enough has been advanced this evening to give us better hopes for the future, and it was to add my quota of experience on the matter that I appear before you. The idea of a recovery from hydrocephalus, when it has advanced to all but the last stage, is not mine, though I am not able to refer you to an authority in favour of this view, but it has been so little recognized as to be worthy certainly of farther consideration.

And now, in conclusion, a few words as to treatment. I believe, then, that if the case be seen early, and the nature of the disease, already spoken of, be kept ever in view, treatment holds out a reasonable prospect of success. But to be really useful it must be steadily carried out. By this I mean that one part of the treatment should be made to support another. Once, in fact, a plan is adopted, it should be continuous. I know no disease which requires this rule more than the one under consideration. As surely as the rule is broken—that is, that there are too long intervals between the several measures used, so surely will the disease gain ground,

Keeping this principle then in view, an antiphlogistic treatment should, in the first instance, be adopted. Leeching to the head or nape should very rarely be omitted. The number must, of course, vary with each case, and it has appeared to me better to order a small number twice than the whole number at once. With the leeches purgatives will be also given; and the compound used by Cheyne, consisting of calomel and jalap, is not to be despised. When the proper period arrives, blistering is most useful, but let it be done with a will; for it has happened to me on different occasions to see a blister the size of a crown put on a child's head who had the disease. This is not the way to conquer such an affection, and is only losing precious time. Let the whole head be blistered, and the surface dressed with some irritating ointment, and then good may be done.

For so far I have spoken of measures long known and used; but, I need not add, with anything but satisfactory results, and yet I look upon this part of the treatment as most essential, but it is not to be trusted to alone. And here it is, it appears to me, a decided advance has been made in the treatment; for, while the measures detailed are being put in force, I now give wine at the same time, according to the age of the child. Whether this plan has been adopted by others I cannot say; I do not know of such. It was gradually that it forced itself on my mind, but of its value I have not now the slightest doubt. All the cases given in this paper, which recovered, were so treated, and I may state that the last detailed got  $\frac{3}{4}$ vi of wine daily, for a period of twelve days, and this in a child of only twelve years of age. No one, however, will jump to the conclusion that all cases recover because wine is given; this is not what I would convey, nor could it in reason be expected, but only that it holds out the best prospect of success, and has, I firmly believe, been directly instrumental in saving life. It will be observed too that the use of the wine should be begun as early as the case and symptoms allow. In this way, whilst we combat the inflammatory symptoms by antiphlogistics, we, on the other hand, sustain the system by the wine—so that the plan is a union of the two. And still keeping in mind the essence of the disease, it seems to me to have everything to recommend it, in addition to the experience detailed this evening, and to which more could have been added.

Of particular medicines I have little to say. Mercury, so much in use formerly, I now use only as an alterative or occasional

purgative, and seldom, in the former way, beyond the second or third day. I had seen a large number of cases where its specific action was induced, and yet I have notes of but one recovery under these circumstances, and now trust to other means. The medicine which seems to me to answer best is the hydriodate of potash. I had read of its use in these cases, and am sure I have seen benefit from it.<sup>a</sup> Before that time I had myself given other salts of potash, with the idea that their diuretic action would be useful; and one very striking case recovered while the child was getting wine and acetate of potash. To each dose of the hydriodate I also usually add from three to five drops of the tincture of digitalis, as being a medicine which Cheyne considered of value, and used accordingly.

In conclusion, then, I have only again to repeat that the union of the two plans of treatment, the antiphlogistic and the stimulant, are the means on which I now mainly rely in this most intractable disease, and all the cases which I have seen recover have been so treated.

ART. IV.—*On Hospital Dietaries.* By JOHN BEDDOE, B.A., M.D., F.S.S.; Physician to the Bristol Royal Infirmary.

THE practical application of dietetic science and experience to the victualling of large bodies of men has of late years been frequently a subject of discussion; but such discussion has almost always been set on foot either by some supposed shortcomings in the feeding of soldiers or of paupers, or by real or apparent excess in that of criminals. The experience of our hospitals has seldom, if ever, been called in to throw further light on these matters; nor am I aware that any general view of hospital dietetics has been put forward, except in a short paper by Dr. Steele, late of Guy's, in the *Social Science Transactions* for 1862. It will be my aim, in the following pages, not so much to insist on the importance of dietetical considerations in therapeutics, as to utilize the practical knowledge of the subject embalmed in the diet-scales of our infirmaries and lunatic

<sup>a</sup> This medicine has, I find, been recommended by several; amongst these are the names of Maunsell and Evanson, Copeland, Willshire, Leney, and Wood of Philadelphia. But none seem to have used it so largely as the late Dr. Coldstream.—(See a paper of his in the *Edinburgh Monthly Journal* for December, 1859.)

asylums,<sup>a</sup> and to point out a line of investigation whereby it may, perhaps, be turned to better account.

In framing such diet-scales regard must be had to three principal considerations. Of these the advantage of the patients stands, of course, first; economy is the next head; and the third, which is intimately connected therewith, is that of facility of administration.

Let us take the least important first. It has always been a question, about what point the multiplication of regular diets becomes more perplexing and troublesome to the physicians and the commissariat than the necessity of continually superadding "extras." The difference of opinion on this point is well shown by a glance at the respective scales of a few hospitals. Of regular permutations the Bristol Infirmary has four; the Bristol General Hospital has three, or six if we reckon the half diets separately; the Bath United and Worcester are contented with only three; while at Birmingham the medical officers may choose among six, called respectively fever diet, milk, broth, and fish diets, ordinary and extra meat diets. In the scales of certain of the Dublin hospitals, with copies of which I have been favoured by Dr. Kidd, the number of variations never exceeds four; but in the Dublin and Cork workhouse infirmaries, where the abolition of the "extra" system seems to be aimed at, it rises to six, seven, twelve, or upwards, without reckoning those formularies which are meant to apply to children alone.

For myself, I prefer, as more convenient, the principle of the Birmingham scales, though to the manner in which the principle has been carried out I do not mean to subscribe. But it is clear that many physicians are of a different opinion, in favour of which they would allege that though the varieties and idiosyncrasies of peptic ability and need which we meet with in private practice are innumerable, yet almost all hospital patients may most conveniently, though roughly, be classed under three or four schedules.

Of these the first would be formed of patients with acute diseases. These are the proper subjects for *low*, *spoon*, or *fever diet*, which consists usually of little else than bread, which bread forms a sort

<sup>a</sup> I take this opportunity of returning thanks to a number of gentlemen, physicians, secretaries, or governors of divers public institutions, who have kindly furnished me with full particulars of the composition of their diet-rolls; and among these I ought to make especial mention of Dr. R. W. Falconer, who has supplied me with the dietaries of most of the principal prisons in England. I can only regret that Dr. Falconer, having with much trouble made and annotated this collection of statistics, had not sufficient leisure to utilize them to better purpose than I may do.



of *substantia* or material basis whereon may be built up the various accidents of extra diet. Such patients are to be found almost exclusively in the medical wards. Of thirty-five cases on low diet in the Bristol Infirmary only four were surgical. Every one of the thirty-five had some extra or extras allowed him.

Patients with chronic diseases might be ranged, for our present purpose, under three heads. The first (and least numerous in most hospitals) consists of persons afflicted with certain cerebral, hepatic, and cutaneous diseases, often connected with the lithic acid diathesis. For these the *broth* and *milk diets* are generally appropriate. Dyspeptic asthmatics do not come under this head; they generally require a higher quality, but a smaller quantity, of food.

Next comes that large class of chronic cases, often of traumatic origin, in which there is no danger of either plethora or exhaustion, in which we wish neither to starve nor to fatten our patients. This class is particularly numerous in surgical wards, and usually receives what is called the *middle* or *ordinary* diet of the hospital.

The fourth class consists of patients with chronic exhausting diseases, of which phthisis furnishes the best type in the medical, and certain strumous affections, or extensive suppurating wounds in the surgical wards. These require the *full* or highest diet, to which stimulants are often advantageously added.

Convalescents would on this plan hardly require to be classed by themselves: they might often be passed with advantage through all the four diet classes above laid down. If classed at all, they might be divided into those with healthy, and those with disordered digestive and excreting organs. Towards the close of their sojourn in hospital the former should, as a rule, receive the full diet; while the latter, of course, require special and individual consideration. I shall return to the dietetic treatment of convalescents in the latter part of this paper.

Though economy is a consideration very subordinate to that of the patient's benefit, it is one of great importance—one which the officials of hospitals supported by voluntary contributions will find to be demanded of them by the public, even if they themselves do not perceive its moment—one, however, which seems to me to be neglected in certain instances which might be designated. I shall presently point out in figures the great difference between the full (which being, according to Dr. Steele, allowed to two-thirds of the patients, is in effect the ordinary) diet of St. Bartholomew's, London, and the diets most in use in the several country hospitals;

and I think the reader will agree with me that either we country physicians starve our patients, or the St. Bartholomew's men must be rather too liberal to theirs in the costly items of meat and beer. This brings me to the question whether beer should form an integral part of the regular diets in any hospital. I am very decided in the opinion that it ought not to do so, but that it should be ordered as an extra where required. This opinion I base partly on moral grounds, as I am satisfied that a considerable obstacle has been opposed, of late years, to the progress of temperance among the working classes, owing to the spread among them of erroneous or exaggerated ideas as to the favour with which strong drinks are regarded by medical men. And what could be more likely to strengthen these ideas than the fact of beer being allowed, as a matter of course, to every patient not on the lowest diet, as is the case at Worcester and at Bath? At Bartholomew's every patient on the usual diet receives two pints of beer daily, and in several other metropolitan hospitals beer is allowed as a rule, though in University and King's such is not the case.

The absence of beer from the formularies of Scotch and Irish hospitals and lunatic asylums has less weight, as a fact, on the anti-alcoholic side of the question, than it otherwise would have, when taken in connexion with its limited use by the working population of the sister countries; and the same may be said of another fact, which I will nevertheless adduce, viz., that the dietary framed for the Scottish prisons, by Drs. Christison and Maclagan, under the use of which the standard of health among the convicts has been remarkably high, and the mortality exceedingly low, contains no form of alcohol. The reasons for giving beer a place in the dietary are probably much stronger in the case of lunatic asylums than in that of general hospitals; and it has a place in the tables of all the county and borough asylums in England and Wales (at least of all those whose tables are published by the Lunacy Commissioners) except five. These five are Cumberland, Durham, Cornwall, Hull, and Haverfordwest. In all these, solid and other liquid foods are given in abundance; and if the absence of beer has any bad effect, which I do not suppose, it certainly does not manifest itself in the statistics of deaths and recoveries, which are rather more favourable than the average.

In both the hospitals of Bristol beer is considered as an extra; and that it is not thought generally necessary or desirable by the officers of the Royal Infirmary, is proved by the fact that on a

certain day I found only twenty-one per cent. of the patients in receipt of any form of alcoholic liquor. With respect to economy, alcohol, if it be a food at all, which I myself do not disbelieve, though some have lately denied it, is a very dear form of food, though in some cases it may be a cheap and efficient medicine.

In attempting to compose model dietaries we must pay attention to the tastes, and even to the prejudices, of the probable recipients of the food. The definition of man as a cooking animal hardly includes the lower class of English among the human species. Their range of taste is generally small; and it would be useless to travel beyond its limits, or to attempt to run counter to their prejudices. Thus I find that the Bristol poor always prefer a small portion of meat to a larger portion of soup or other food, though the latter may be both more savoury and more nutritive. Dr. Burder informs me that bread puddings were discarded at the Bristol General Hospital in deference to the repugnance of the patients to what they supposed to be partly composed of scraps; and the very same thing occurred at the Infirmary. There is seldom much craving for variety; but even though that craving be absent, I doubt not that a greater variety from day to day than is allowed in most hospitals would be productive of good, and that more perfect digestion, involving a true economy of nutriment, would accompany the greater gratification of the palate. In most lunatic asylums, and in many prisons, this point is attended to more carefully than in hospitals, being thought more important in these two classes of institutions, on account of the greater duration of the confinement of their inmates, and of their exposition to the influences of a regulated dietary.

I will now proceed to the investigation of the quantity of food, or rather of the quantity of nutritive elements contained in the food, allowed in our diet-scales; its relation to that given in workhouses, lunatic asylums, and prisons, and to that consumed by private individuals; and further, to the consideration of the important question, how much food ought generally to be allotted to the average hospital patient and to the average convalescent.

This part of my subject is beset with numerous difficulties, some of which it is not possible entirely to remove. I have just now spoken of the average hospital patient; but it has been well said that no such thing exists as "the average man," and that no one man is exactly like another, whether in external form or internal constitution. Hospital physicians, of course, recognize and act upon this

fact to a degree which would not be practicable in some other public institutions; nevertheless we are obliged, to some extent, to follow the system of Procrustes; and as he stretched or cut down his patients to the length of his bed, so we must sometimes make ours suit themselves to the diet, rather than suit the diet to them. It is incumbent upon us, therefore, to determine as nearly as possible the value of this imaginary quantity, the average hospital patient considered as a consumer of food.

The greatest difficulties arise from the, as yet, rudimentary state of physiology and animal chemistry, from our imperfect knowledge of the details of the processes of digestion and sanguification, and still more those of destructive metamorphosis. How little do we, as yet, positively know, however much we may conjecture, of the forces which regulate the amount of destructive changes that go on in the human body; of the sources of urea, uric acid, and kreatin, and the circumstances which determine their amount, and which define the quantities of nutriment required to trim the balances of bodily weight, and of muscular and nervous power. In our present ignorance or uncertainty about these things it is difficult, if not impossible, to explain the enormous discrepancies which occur in the consumption of food by different individuals. How was it that Cornaro maintained, as we have read and believe, not only life, but some amount of bodily and mental vigour, for sixty years, on twelve ounces of solid food, and fourteen ounces of wine daily? Or if this may possibly be accounted for by a judicious selection of the most concentrated food, how shall we explain those cases, which almost all of us must have seen, in which animal heat and plumpness, if nothing more, have been kept up, for long periods, on quantities of food which *appear* wholly insufficient, and under circumstances in which, though deception may be suspected, we are convinced that if any nutriment be really smuggled clandestinely into the system, its amount must be but small? Many cases of great and habitual excess in eating, though less wonderful, are also incapable of perfect and satisfactory explanation in the present state of our knowledge; and, at the risk of being set down as a sceptic, I must confess that it is not easy to reconcile them with the current theory on the production of animal heat.

Great difficulties also arise, in forming or comparing diet tables, from the varying composition of sundry kinds of food, and from the fact that some kinds either have never been chemically analysed, or their analyses have been imperfectly executed, or incorrectly



reported. I will mention one or two examples of these difficulties.

Soups and puddings enter more or less conspicuously into most diet-scales, but even where the nature of their several ingredients can be exactly ascertained, the respective quantities can seldom be so, being for the most part unknown to the very concoctors themselves.

Again, the variable proportion of fat in meat causes wide differences in its nutritive value, differences of the greatest importance, but usually much undervalued. The quantity of fat in ordinary meat was by most writers on diet greatly underrated, and by some, wonderful to be said, its presence was wholly ignored, until Lawes and Gilbert showed that meat, especially mutton and pork, fattened to the gross and unnatural degree which is so common now-a-days, ceases to be a pre-eminently nitrogenous food in becoming a highly carbonaceous one. Judging of the proportions by the eye, and from some rough experiments and calculations,<sup>a</sup> I have estimated the cooked meat of the Bristol Infirmary to contain 25 per cent. of pure, dry, lean, or nitrogenous matter, and 20 of fat; and this estimate I have applied to other hospitals; but it is probable that in prisons and workhouses coarser and fatter meat is generally used; and as the meat is often cooked by boiling, 22·5 nitrogenous, and 22·5 carbonaceous would, in those cases, be nearer the mark. In fact, however, the waste of fat varies so much with the mode of cooking and the taste of the consumer, that any estimate of this sort must be often wide of the mark. It is extremely difficult to set down the probable value of broth, beef-tea, &c., even where they are not complicated by the addition of vegetable ingredients, and so converted into soup. The beef-tea of the Bristol Infirmary is rather a gelatinous broth than a true beef-tea, and its proportion of animal to saline matter is, as will be observed in the subsequent tables, rather high.

Even in the article of wheaten bread analyses differ very much.

<sup>a</sup> The carcass of Lawes and Gilbert's "half-fat ox" contained per cent. about 13 of bone, 15 of dry nitrogenized matters in the flesh, 20 of dry fat, and 1 of ash, (in "fat" oxen, and even in store sheep, the proportion of fat was greater.) Assume then that the raw meat of the Infirmary, consisting chiefly of legs of mutton, contains 10 per cent. of bone, 15 of nitrogenous, and 20 of fatty matter. If it loses in roasting 8 per cent. of dripping and 22 of water, there will remain 60 of meat, containing 15 nit. and 12 fat, or per cent. 25 and 20. The loss of dripping is often greater; in boiling, on the other hand, less fat is lost to the meat, but a notable proportion of nitrogen goes into the broth.

Christison, in his earlier calculations at least, assigned to it  $10\frac{1}{2}$  per cent. of nitrogenous matter; but Maclagan found an average of less than 8 per cent. in Edinburgh bread. I think these differences depend mainly on the quality and country of growth of the wheat; Lawes and Gilbert, operating on bread made from English flour, found a little more than 8 per cent. Dr. Griffin, of this city, analysed for me two samples of good seconds flour employed in the Bristol Infirmary, and found therein an average of nearly 1.99 per cent. of nitrogen, which indicated, according to my calculations, the presence of fully 9 per cent. of plastic matters in the Infirmary bread. I am informed that the difference of texture in bread and flour renders it somewhat more difficult to ensure perfect combustion in the case of the former; and this may possibly account for some analyses, in which dry bread appears to contain less nitrogen than the flour of which it is made taken in the same dry condition. I have thought it safest to assign to bread 8.5 per cent. of nitrogenized constituents.

Arithmetical blunders, and want of practical acquaintance with the subject, have resulted in the publication of some curiously erroneous analyses. Milk of all sorts has been examined many a time, but nobody seems to have thought it worth his while to make an analysis of cream, until Professor Völcker, of Cirencester, supplied the void. The illustrious Berzelius had once published a partial one of an ordinary specimen of *milk*. I do not know whether the great chemist's pen had slipped, or whether his English translator had a bad dictionary, but so it fell out that the analysis was published in England as referring to *cream*. Pereira copied it into his work on diet; and from that time to this almost every writer on the subject has done the same, down to Dr. Dobell, who published a useful analytic table a few months ago. One *savant* only, who had probably been outside of London, and seen that the cream was at the top of the milk, seems to have been struck with the singularity of a cream containing only  $4\frac{1}{2}$  per cent. of butter, and he qualified Berzelius' fluid as "a specimen of *thin* Swedish cream!" I myself found, in an ordinary sample, 27 per cent. of butter, or six times as much as was set down by these authors, none of whom, I suppose, had ever heard of the rough farmer's rule, "a quart of cream to a pound of butter." Dr. Völcker found 25.8 per cent.

For the sake of perspicuity, in the face of all these doubts, mistakes, and difficulties, I have thought it necessary to prefix to my

chemical analysis of dietaries a standard table of the composition of food. It is on a similar plan to that of Dr. Dobell; but it will be observed that in some of its items it does not exactly correspond with his or any other such table. In framing it I have consulted a great number of authorities, British and Foreign, not forgetting, of course, to delve in the mine of Moleschott. Where I found great diversity of statements I have not contented myself with a simple average of the whole, but have often given more weight to the dicta of individual analysts who seemed to deserve it.

The last difficulty I shall dwell upon is, that practical proof of the *perfection* of a diet-scale is unattainable. It is not enough to say "so many people, such and such bodies of men, have lived in this way for a length of time and found it answer very well." It may be so; but a different regimen, if tried, might have answered still better: who can say? Whole nations go on, year after year, and generation after generation, committing gross dietetic errors without finding them out. I suppose the Westmann islanders would never themselves have discovered that their diet of sea-fowls' eggs and stinking fish had anything to do with the death of all their infants from trismus. The Färoers are said to prefer their food putrid; yet most of them live on it to a good old age, though it entails on them habitual diarrhea. Norfolk dumplings are accused of producing the stones in which that county is so fertile; and Balassa, the eminent Hungarian surgeon, speaking to me of the frequency of both cataract and calculus among the peasantry of his country, was disposed to ascribe it to their diet of boiled flour and grease. In the northern parts of this island, at the present day, the mass of the population is gradually, voluntarily, and without any other motive than taste or fashion, deserting a better for a worse, a stronger for a weaker diet; and the substitution of white bread and tea for oatmeal and milk will probably soon manifest its effects in a notable deterioration of the breed.

Standard Table of Composition of Food.

	Nitrogenous	Fatty	Amylaceous	Indig. or Complem.	Mineral	Water
Bread, - - - -	8·5	1·	52·	1·	1·5	36·
Wheat flour, - - - -	12·	1·25	72·	1·	1·	12·75
Oatmeal, - - - -	16·5	6·	60·	2·5	2·5	12·5
Maize-meal, - - - -	11·	7·	65·	3·	1·	13·
Rice, - - - -	6·5	·5	80·	1·	·5	11·5
Pearl barley, - - - -	9·	1·	75·	1·5	1·	12·5
Peas, - - - -	22·5	2·	50·	9·	2·5	14·
Potato, - - - -	1·75	?	17·5	4·5	1·25	75·
Do., cooked and skinned, -	2·	?	20·	2·75?	1·25	74·
Carrot, - - - -	1·5	·2	8·5	3·5	1·3	85·
Other vegetables, average estimate, -	1·	—	5·	—	—	—
Meat, cooked, boneless (hospitals), -	25·	20·	—	—	2·	53·
Do., do., (prisons), -	22·5	22·5	—	—	2·	53·
Do., with bone, - - - -	20·	20·	—	13·		47·
Do., raw, bony, - - - -	15·	20·	—	10·		—
Do., raw, boneless, - - - -	16·6	22·5	—	—	1·	60·
Bacon, - - - -	8·	75·80	—	—	?	?
Bones (varies with mode of cooking), -	—	—	—	—	—	—
Egg (calculated on two ounces), -	12·5	10·	—	—	1·	76·5
New milk, - - - -	4·	3·5	4·5	—	·5	87·5
Skimmed milk, - - - -	4·	·5	4·5	—	·5	90·5
Cheese, - - - -	30·	27·	3·	—	5·	35·
Butter, - - - -	2·5	87·5	—	—	2·	8·
Suet, - - - -	1·	95·	—	—	—	4·
Sugar, common raw, - - - -	1·	—	92·	—	1·	6·
Cocoa, - - - -	10·	50·	20·	—	?	?
Beer, average, - - - -	·05	—	6·	Alcohol	?	?
Beef-tea, - - - -	3·	—	—	—	1·	96·



WEEKLY																				DAILY				Informant
																		Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total			
		Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar or Molasses, oz.	Cocoa, oz.	Milk, oz.	Beer, pints.	Extras					
Bristol Royal Infirmary (full), Do. Do.		84	—	—	—	—	—	—	42	—	—	42	—	—	—	—	—	70	—	—				
		84	—	—	—	7	—	—	21	—	—	21	—	—	1·5	—	140	—	—	—				
		70	—	—	—	21	—	—	—	7	—	—	—	—	1·5	—	140	—	Soup, bones	—				
Bristol Gen. Hospital (extra), Do. Do.		126	—	7	—	—	—	—	42	—	42	—	—	—	—	—	—	70	—	—				
		84	—	7	—	—	—	—	28	—	—	28	—	—	—	—	70	—	—	—				
		84	—	7	—	—	—	—	—	—	—	—	—	—	?	—	70	—	Soup and pudding	—				
Worcester Do.		98	—	—	—	—	—	—	48	—	18	—	—	—	3·	Tea	16	7	Bth. soup	—				
		98	—	—	—	—	—	—	48	—	24	—	—	—	3·	Tea	16	7	Bth. soup	—				
		84	—	—	—	—	—	—	32	—	16	—	—	3·5	10·?	Tea	80?	7	Pudding 3 days	—				
Bath United Do.		84	—	—	—	—	—	—	48	—	24	—	—	3·5	7·	Tea	64	7	Pudding 1 day	—				
		84	—	—	—	—	—	—	44	—	32	—	—	7·6	4·5	Tea	17	4	Soup 3 days	—				
		112	—	—	—	—	—	—	56	—	28	—	—	3·	—	Tea	35?	—	—	—				
Taunton		112	—	—	—	—	—	—	56	—	42	—	—	3·	—	Tea	35?	—	—	—				
Do.		112	—	—	—	—	—	—	56	—	42	—	—	3·	—	Tea	35?	—	—	—				

	WEEKLY														DAILY			Informant	—
	Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Pears, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar or molasses, oz.	Cocoa, oz.	Milk, oz.	Beer, pints	Extras	
Birming, Queen's Hosp. (ord.),	84	—	7	—	—	—	—	56	—	42	—	—	—	3½	Tea	79	—	—	Dr. Fleming
Do., (extra),	84	—	7	—	—	—	—	56	—	84	—	—	—	3½	Tea	79	14	—	Do.
Bartholomew's, (full),	98	—	—	—	—	—	—	56	—	—	56	—	7	5¼	Tea	—	14	—	—
Glasgow, (ord.),	98	—	—	—	—	18	—	—	Bones Vary	14	23	—	7	8¾	Tea	12	—	—	Mr. Thomson
Do., (milk),	56	—	—	—	17½	—	—	—	—	—	—	—	—	—	—	420	—	—	Do.
Edinburgh, (common),	84	—	—	—	—	7	—	112	5½	14	—	—	—	—	Tea	21	—	—	Mr. M'Dougall
Do., (steak),	84	—	—	—	—	7	—	112	5½	42	—	—	—	7	Coff. Tea	21	—	—	Do.
Do., (full),	21	—	31½	—	—	7	—	224	5½	56	—	—	—	—	Coff. 140	70	—	—	Do.
Dublin, Richmond, Whitworth, Hardwicke, (mid. with fast),	112	—	3½	—	—	2½	—	—	2½	20	—	—	—	7½	Tea	17½	—	—	Mr. M'Mullen, per Dr. Kidd
Dublin, Meath, (mid. with fast),	112	—	27	—	—	—	—	—	Herbs	—	—	—	—	3½	Tea	146	—	Broth	Mr. Anderson, per Dr. Kidd
Dublin, R.W.H., (mutton),	112	—	—	—	—	—	—	—	—	56	—	—	—	7	Tea	17½	—	—	—
Do., (full with fast),	116	—	4½	—	—	5	—	48	5	40	—	—	—	7½	Tea	17½	—	—	—
Dublin, Meath, (full with fast),	112	—	27	—	—	—	—	—	Herbs	40	—	—	—	3½	Tea	146	—	—	—

Nitrogenous, oz.

Fatty, oz.

Amylo-saccharine, oz.

Total oz.

—

Informant

WEEKLY													DAILY				Informant						
Bread, oz.	Flour, oz.	Oatmeal, oz.	Malt-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c. oz.	Sugar, Molasses, &c., oz.	Cocoa, &c., oz.	Milk, oz.	Beer, pints		Extras	Nitrogenous, oz.	Fatty, oz.	Amylo-saccharine, oz.	Total	
North Dublin Union, Convalescents (No. 2, 2, mutton, one of best), .	132	—	—	—	—	—	—	—	—	36	—	—	—	5½	—	157½	—	—	3·66	2·13	12·93	18·72	Dr. Minchin, per Dr. Kidd
Do. (Appendix No. 3-5, best of infirm and epileptic ds., .	84	—	31½	10½	—	—	—	—	—	—	—	—	—	—	—	210	—	—	3·13	1·52	11·26	15·91	—

In those hospitals in which tea, sugar, and butter are not given, the patients are usually allowed to introduce them themselves; this is the case at Bristol, where the amounts in the second and third columns therefore appear rather too low.

LUNATIC ASYLUMS.													Coff.				Coff.				Coff.				Mr. Buck	Dr. Thurnam	
Sussex County, .	126	5	—	—	—	—	—	32	36	36	—	—	3½	11½	3½	56	3½	—	—	2·85	2·86	13·64	19·35	—			—
Do., workers, extra,	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	—	—	3·26	3·11	15·58	21·95	—			—
Leicestershire and Rutland,	108	6	¾	—	—	—	66	7	24	25½	6½	—	—	—	—	43½	3	—	—	2·94	2·10	11·83	16·87	Mr. Buck			—
Do., workers, extra,	24	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	—	—	3·50	2·37	14·52	20·39	—			—
Wilts County, .	98	9½	—	—	—	—	—	65	—	16½	14·5	5½	—	—	—	17½	9½	—	—	3·01	2·11	12·70	17·82	Dr. Thurnam			—
Cumberland, .	78	12	42	—	—	—	—	50	—	12½	12½	25½	1½	—	—	312	—	—	—	4·91	3·86	14·43	23·20	—			—

WEEKLY														DAILY				Informant					
Bread, oz.	Flour, oz.	Oatmeal, oz.	Maize-meal, oz.	Rice, oz.	Barley, oz.	Peas, oz.	Potatoes, oz.	Vegetables in gen., oz.	Meat, raw, oz.	Meat, cooked, oz.	Cheese, oz.	Butter, suet, &c., oz.	Sugar, Molass, &c., oz.	Cocoa, oz.	Milk, oz.	Beer, pints	Extras		Nitrogenous, oz.	Fatty, oz.	Ammono-saccharine, oz.	Total	
Armagh District Asylum, .	64	63	—	—	3·5	—	48	63	32·6	—	—	—	—	—	225	—	—	—	4·55	2·71	13·74	21·00	Dr. M'Kinstry
Do., workers extra,	48	—	—	—	—	—	—	—	—	—	—	—	Tea	—	—	—	—	Bones 18	5·15	2·78	17·38	25·31	—
Sligo and Leitrim, .	82	440½	28	6	—	—	50½	—	26	—	—	—	1	—	160	—	—	—	4·31	2·65	16·46	23·42	Mr. M'Munn
Abergavenny, . . .	114	—	—	—	—	1½	100	31	5	5·5	8	—	—	—	20	5½ or added	—	—	2·73	2·29	12·80	17·82	Dr. M'Cullough
Do., workers extra,	12	—	—	—	—	—	—	—	—	6	—	—	—	—	—	6	—	—	3·14	2·54	14·74	20·42	—

In almost all lunatic asylums there is an extra allowance for workers; but I have not given this in every case.

WORKHOUSES.																							
Clifton Workhouse, .	113	9	13½	4	—	6	48	—	—	9	10½	2	—	—	52½	—	—	—	3·29	1·61	13·16	18·06	—
Bath Workhouse, .	132	—	12	—	—	—	3	24	—	—	13	18½	—	—	—	—	—	—	3·22	1·82	11·81	16·85	—
Cork Union, . . .	112	—	10½	56	4	—	—	—	21?	—	—	—	—	—	105	—	—	—	3·24	1·	14·73	18·97	—

In almost all lunatic asylums there is an extra allowance for workers; but I have not given this in every case.

## WORKHOUSES.

Clifton Workhouse,	.	113	9 13½	—	4	—	6	48	—	—	9 10½	2	—	—	52½	—	—	3·29	1·61	13·16	18·06	—
Bath Workhouse,	.	132	—	12	—	—	3	24	—	—	13 18½	—	—	—	—	—	—	3·22	1·82	11·81	16·85	—
Cork Union,	.	112	—	10½	56	4	—	—	21?	—	—	—	—	—	—	—	—	3·24	1·	14·73	18·97	—





—	Nit.	Fat	Amy.	Total
Six male Lancashire operatives, during "cotton famine," calculated from E. Smith's data, . . . . .	3·17	1·94	16·54	11·55
A very poor Northampton shoemaker, also from E. Smith's data, . . . . .	2·27	2·05	11·39	5·71
A very poor Staffordshire labourer, do., do., two-fifths of consumption of man, wife, and two small children, .	2·18	1·62	12·68	16·48
A Gloucestershire labourer, do., do., one-half con- sumption of man and wife, . . . . .	4·27	2·43	25·	31·70
Military hospitals, low diet, . . . . .	2·65	2·50	11·15	16·30
Do., half diet, . . . . .	3·34	2·	14·14	19·48
Do., extra diet, . . . . .	4·17	2·80	15·81	22·78
Leicestershire Asylum, male attendants' diet (strictly ad- hered to, but alcohol in addition), . . . . .	3·96	3·42	14·52	21·90

Perhaps the most obvious conclusion to be drawn from the foregoing tables is that the prison diets are generally more liberal in quantity than those of the lunatic asylums and workhouses, while those of the hospitals, as a rule, stand last. It may be that the prison diets, examined from a moral and economical, as well as a physical point of view, may be generally too high, or at least too large in quantity; but so far as their effects on the health of the convicts are concerned, there is probably very little fault to be found with them. In the Scotch prisons, for example, the rate of mortality is low, and the standard of health high; and in certain experiments made in Morpeth Gaol (the most usual dietary of which I have selected for examination) it was found that convicts exactly maintained their initial weight. Allowing, however, for the partial failure of digestive power which is reported to be frequent among prisoners, it is probable that these prison dietaries are more copious than would be necessary for other persons undergoing similar amounts of exertion. There are great differences as respects quantity among the lunatic asylums—so great that one is led to suppose, at first sight, as in the cases of certain hospitals before mentioned, that there must be deficiency or excess in one or both of the extremes. The investigation of the matter is a little complicated by the presence of beer in the ordinary diet scales of most of the English asylums, including all those where deficiency in other respects might be

suspected. The scale is remarkably low at Abergavenny, where it works very well, and where, Dr. M'Cullough informs me, there can be no question that the patients are well nourished, and in good condition; and it is not much higher at Leicester and Devizes, where Mr. Buck and Dr. Thurnam find the quantities sufficient. The latter, indeed, reports to me that the Wiltshire labourers are apt to get fat on their asylum allowances. In the Irish asylum diets the quantities appear rather large; but they are exceeded in amount of carbon by some English ones.

Dr. Edward Smith's official reports to the Privy Council might be expected to afford much valuable information as to the minimum amount of food necessary for the sustentation of health; but only a small portion of this large collection of facts is available for my present purpose. His reports of the dietary of independent labourers are necessarily almost always complicated by the presence of women and children in the families; moreover, the ideas of the labouring class, generally, in England, as to their expenditure for food, are so very hazy, that we cannot but regard the reports with great suspicion. It is, in fact, a matter of extreme difficulty to ascertain what quantities of food are ordinarily consumed by that class of persons which furnishes the inmates of hospitals; but there is no doubt that if most of them do not eat much more when at home in health than when in hospital, "it is their poverty, and not their will" that consents to this temperance. I have computed the nutriment in two or three of Smith's cases, where a very varied amount of nourishment seems to have concurred with the maintenance of health. But I have much more confidence in his account of the diet of six male Lancashire operatives, during the recent depression of the cotton trade, which also I have computed according to the system adopted in this paper. It ranged much higher than our infirmary, or even our workhouse diets; but the excess was chiefly or altogether in amylaceous food. This observation, which appears to have been carefully made, has the more value as we now know that the population of Lancashire, generally speaking, was sufficiently well fed during the "cotton famine" to maintain a fair standard of health, though the quantity and quality of their food were both below what was customary with them. I have myself ascertained the consumption of a family of six adults, not stinted by lack of means, to be about 3·8 oz. nitrogenous, 2·9 fatty, and 12·5 amylaceous, per head, per diem, which about equals the full diet of St. Bartholomew's, but considerably exceeds most hospital

diets. The average amount of nitrogenous food obtained by outdoor labourers and their families in England, is, if we accept Edward Smith's reports, about 3·5 per diem, per computed adult. But while on the one hand, persons of sufficient income and active habits generally take a very much greater quantity of nitrogenous food without any apparent detriment, the body when in perfect rest certainly requires very much less. The experiments of Gasparin and of John Ranke (Parkes on Hygiene, p. 138), certainly yield results, as to nitrogen, too high to be generally applicable. Ranke required 240 grains nitrogen (= 3·47 oz. nitrogenous matter), and 3,531 grains carbon (= 8·04 oz.), to exactly maintain his weight, and the balance between ingesta and egesta. But in the first place these figures should be considerably reduced to render them applicable to ordinary men, as this physiologist has a remarkably tall and fine frame; and in the second, it seems probable that when a man accustomed to live plentifully, submits to a disagreeable degree of abstinence, he "frets his tissues" to a degree that may vitiate the experiment.

The question may now be put—are the quantities allowed in our hospital diet-scales sufficient? There are several undeniable reasons why they should be smaller than in most of the dietaries with which I have compared them. Of these, the comparative want of exercise, and consequent lessened waste of the tissues, is a chief one, to which we may add in winter the lessened exposure to cold. Again, we have the power of supplementing a diet which appears relatively deficient by extras;<sup>a</sup> and there may therefore be much less risk in a deficient than in an excessive standard. Thirdly, it is of course wrong to judge of the nutritive quality of food exclusively by its chemical composition; and the food of an infirmary, of better quality and better cooked than what working men commonly obtain, may prove more nutritive, because more tempting and digestible, than a large quantity of coarse or insipid aliment. It is curious that the most liberal dietary (chemically speaking), of those which I have analyzed, is that of Usk gaol, consisting chiefly of Indian meal and oatmeal, with milk; but though it would probably have sufficed to maintain the vigour of a hardy Scotch peasant

<sup>a</sup> In military hospitals it is the endeavour to do away, as far as possible, with the system of extras (Parkes on Hygiene); in accordance with this aim, it will have been observed, that the dietaries there are much higher than in civil hospitals. Another motive for this liberality in scale is the desire to fit the convalescents for duty as soon as possible.



under much exertion and exposure, I am informed by Dr. Falconer that it was not found to suit the health of prisoners long in confinement; and, I believe it was not its excessive quantity but its insipidity and, potentially viewed, its scantiness, which was thought to be in fault. It contained no meat, but the quantity of proteineous nutriment was far greater than in an ordinary meat diet; and when it was improved in favour of certain classes of prisoners, by the substitution of a portion of meat and potatoes for meal and milk, it was, according to chemical notation, considerably reduced.

It is now some time since I conceived the idea of testing the sufficiency of the diets of the Bristol Royal Infirmary, by weighing the convalescents and chronic invalids at intervals, and at the same time examining the urine, roughly determining the amount of urinary solids, and thus conjecturing, if not absolutely determining, the extent of waste or deficiency of food. The observations of the urine, and of the weights, have not been conducted with so much scientific accuracy as would have been practicable in clinical wards; but, I trust they are of sufficient interest and value to deserve publication.

*Experiments on Convalescent Diet in Bristol Infirmary.<sup>a</sup>*

McCarthy, forty-nine, fish-hawker, formerly soldier, 5 feet 9 inches; weight, when in good health, 160 lbs.; phthisis in first stage, with marked symptoms; improving. Taking iron and quinine; no butter, but 3vi. of cod-oil daily. January 30th. Weight 148 lbs., diet middle—bread 4 oz., milk  $\frac{1}{2}$  pint, beef-tea  $\frac{1}{2}$  pint, cod-oil 3vi., sugar 1 oz., or  $3\cdot77$ ,  $2\cdot55$ ,  $12\cdot27=18\cdot60$ . February 9th. Weight, 150 lbs. 13th. Urine of 2 days' average, 49 oz., sp. gr. 1017, solids 833 grs., urea (by Haughton<sup>b</sup>) 392. 17th. Weight  $152\frac{1}{2}$  lbs., diet full—milk 1 pint, beef-tea  $\frac{1}{2}$  pint, cod-oil 3vi., sugar 1 oz.  $=4\cdot17$ ,  $3\cdot11$ ,  $9\cdot80=17\cdot08$ . 23rd. (Very cold weather.) Urine of 2 days' average, 106 oz., sp. gr. 1010, solids 1060, urea (Haughton) 544. 24th. Weight 154 lbs. March 1st. Urine of 2 days' average, 66 oz., sp. gr. 1012, solids 792, urea (Haughton) 390, weight

<sup>a</sup> I have made such allowances for butter and sugar as appeared probable. The words *full*, *middle*, *soup*, refer to the diet scales of the Infirmary. The men were always weighed at the same period of the day, and in the same clothes, if possible. No urine was lost at stool; and no patient suffering from diarrhea was included.

<sup>b</sup> I have estimated the urea by Haughton's plan, though I could wish that he would give us more certain ground for relying on it; U (H) signifies urea by Haughton's rule. The greater the quantity of food the greater is that of urophane salts; hence in the convalescents the urea is probably put too high.

155 lbs. March 4th. Weight 155 lbs., diet full—bread 4 oz., egg 1, cod-oil 3vi., sugar 1 oz.=3.66, 2.64, 11=17.30. 9th. 2 days' urine, average 66 oz., sp. gr. (?). 11th. Weight 156 lbs. (?). 16th. 1 day's urine, 48 oz., 1015, solids 720, urea (Haughton) 340. 18th. Weight  $155\frac{1}{4}$  lbs., diet, full—bread 4 oz., egg 1, milk 1 pint, cod-oil 3vi., sugar 1 oz.=4.46, 3.34, 11.88=19.68. 23rd. 1 day's urine, 66 oz., 1016 $\frac{1}{2}$ , solids 990, Urea (H) 467. 24th. Weight  $156\frac{1}{2}$  lbs.

This man had ceased to improve in weight and appearance on the third diet, but when a pint of milk had been added to it began to mend again in both respects, though the urinary solids appear to have increased with the nitrogen in the diet.

Reynolds, twenty-five, shoemaker, 6 feet, healthy weight unknown, convalescent from enteric fever; quinine. December 24th. Weight  $132\frac{1}{2}$  lbs., diet, middle—bread 6 oz., beef-tea 1 pint, milk  $\frac{1}{2}$  pint, butter 1 oz., sugar 1 oz.=4.24, 2.85, 13.30=20.39. 28th. Urine, 2 days' average, 72 oz., sp. gr. 1009, solids 648 grs., urea (Haughton) 360 (?). 30th. Weight 138 lbs., increase of  $5\frac{1}{2}$  lbs. in 6 days. A large diet, but evidently no surplusage; the urinary solids being very low, and the increase great.

Powell, about sixty, healthy weight unknown; had had an ulcer occupying the whole visible portion of the pharynx, which almost wholly prevented him from swallowing even liquids, so that he depended on sucking underdone meat; he had been much emaciated, but the ulcer was almost healed when the observation was begun. Diet full—milk-bread 8 oz., 2 eggs, milk 1 pint, rice pudding 8 oz., beer 1 pint, sherry 2 oz., butter 1 oz., sugar 1 oz.=5.80, 4.20, 15.65=25.65. Increase in weight,  $13\frac{1}{2}$  lbs. in 8 days; urine unfortunately not examined.

This is the most rapid increase I have ever observed, and seems hardly explicable even by the very copious diet. I do not think he got any food without my knowledge. It is possible that an unusual proportion of the increase consisted of water, as he had for many days been unable to take any fluid.

Ch. Williams, nineteen, labourer, recovering from typhoid fever, complicated with pneumonia and pleurisy. January 23rd. Weight 118 lbs., diet middle—extra bread 6 oz., egg 1, beef-tea 1 pint, milk 1 pint, beer 1 pint, cod-oil 3vi., butter and sugar=4.90, 4, 14.90=23.80. 29th. Urine 82 oz., sp. gr. 1016, solids 1312, U(H) 634. 30th. Weight  $119\frac{1}{2}$  lbs; diet on 30th only 3.64, 3.34, 11.78=18.76, urine 60, 1012, solids 720, U(H) 355. 31st.

Urine 60, 1015, solids 900, U(H) 426. February 1st. Urine 48, 1020½, solids 984, U(H) 490. The urine on and after this day was collected and estimated as follows: the urine attributed to each day was taken from the vessel at the close of the evening of that day. February 2nd. Urine 66, 1013, solids 858, U(H) 431, diet 3·64, 3·34, 11·78=18·76. 3rd. Urine 66, 1015, solids 990, U(H) 468, diet 6·06, 4·66, 18·=28·72. 4th. Urine 72, 1014, solids 1008, U(H) 489, diet 4·90, 4·, 14·90=23·80. 5th. Urine 62, 1012, solids 744, U(H) 366, diet 3·64, 3·34, 11·78=18·76. 6th. Weight 125 lbs.; gain 5½ lbs., or 8 lbs. per diem, urine 60, 1014, solids 840, U(H) 408, diet 4·90, 4·, 14·90=23·80. 7th. Urine 62, 1010, solids 620, U(H) 319, diet 6·06, 4·66, 18·=28·72. 8th. Urine 62, 1011, solids 682, U(H) 329, diet 4·90, 4·, 14·90=23·80. Same diet continued. The previous changes had been effected by sometimes withholding and sometimes doubling the 6 oz. extra bread, and the 3 oz. cooked meat. 13th. Weight 129½ lbs.; gain, 10 lbs. in a fortnight on the preceding diet; diet full—extra bread 6 oz., 1 egg, ½ pint beef-tea, ½ pint milk, ½ pint beer (the reduction of beer did not take effect till the 16th), butter and sugar; cod-oil 3vi., or 4·54, 3·90, 12·97=21·41. 14th. Urine 70, 1013, solids 921, U(H) 456. 15th. Urine 78, 1015, solids 1170, U(H) 553. 16th. Urine 68, 1017, solids 1156, U(H) 544. 17th. Urine 84, 1015, solids 1260, U(H) 596. 18th. Urine 74, 1014, solids 1036, U(H) 503. 19th. Urine 72, 1019, solids 1368, U(H) 838, diet reduced by abstraction of 6 oz. bread, 4·03, 3·84, 9·85=17·72. 20th. Weight 134½ lbs.; gain 5 lbs., diet as yesterday continued, urine 76, 1017, solids 1292, U(H) 608. 21st. Urine 60, 1022, solids 1320, U(H) 772. 22nd. Urine 68, 1021, solids 1428, U(H) 847. 23rd. 62, 1020, solids 1240, U(H) 746. 24th. Urine 72, 1015, solids 1080 U(H) 511. 25th Urine 76, 1014, solids 1064, U(H) 516, diet, 2·53, 2·64, 9·85=15·02. 26th. Urine 77, 1015, solids 1155, U(H) 546, diet 5·53, 5·04, 9·85=20·42. 27th. Urine 53, 1019½, solids 1034, U(H) 627, diet 4·03, 3·84, 9·85=17·72; weight 137 lbs.; gain 2½ lbs. 28th. Urine 42, 1020 solids 840, U(H) 506, diet 4·03, 3·84, 9·85=17·72. 29th. Urine 52, 1023, solids 1196, U(H) 692. March 1st. Urine 46, 1021, solids 966 U(H) 573. 2nd. Urine 48, 1021, solids 1008, U(H) 598. 5th. Weight 146 lbs.; gain in 7 days, 9 lbs., in 14 days, 11½ lbs. On the 25th the meat had been withheld, and on the 26th doubled.

I think I am entitled to draw some inferences from this in-

teresting case. During the whole period the patient's pulse was quick and feeble, but gradually becoming less so; he was also inclined to perspire freely, especially in the early part of the experiment, when he was wholly confined to bed. There were suspicions of tuberculosis. I think the comparatively low amounts of solids in the first half of February may have been partly due to excretion of urea by sweating. It would seem that the amount of amylo-saccharine food was larger than necessary at first, for its reduction had no apparent effect on the rate of increase. The effect of the occasional variations in the quantity of meat and bread was probably beneficial; the doubling of the meat had very little influence on the solids of the urine, whether on the same or the succeeding day. I reduced the bread on the 19th February because the patient had for some days appeared "heated" and oppressed, while the urinary solids increased. The reduction had, however, no immediate effect, and perhaps the feverish symptoms were due to some other cause than excess of food.

Maher, fifty-four, labourer, 5 feet 9 inches; chronic nasal catarrh; improving; doubtful syphilitic taint; taking small doses of iod.-ferri and quinine. December 7th. Weight  $151\frac{1}{2}$  lbs., diet middle—extra bread 4 oz., beef-tea  $\frac{1}{2}$  pint, sugar, little butter = 3.37, 1.75 (?), 11.82 = 16.94, urine 2 day's average 55 oz., sp. gr. 1016, solids 880, urea (Haughton) 415. 15th. Weight  $151\frac{1}{2}$  lbs. (unaltered), diet middle—bread 6 oz., beef-tea  $\frac{1}{2}$  pint, milk  $\frac{1}{2}$  pint, sugar and butter as above = 3.94, 2.12, 13.31 = 19.37, urine 2 days' average, 72 oz., 1014, solids 868, urea (H) 421. 21st. Weight  $153\frac{1}{2}$ ; gain 2 lbs. In Maher's case the first diet was probably insufficient; for its increase fattened the man without affecting the urinary solids.

Hill, twenty, brickmaker, of rather large frame; slight rheumatism. February 23rd. Weight  $153\frac{1}{2}$  lbs., diet soup, with butter and sugar = 2.05, 1.85, 10 = 13.90; in bed. 27th. Weight 151 lbs., diet middle—with b. and s. = 2.72, 2.24, 9.64 = 14.58. March 5th. Weight 151 lbs., urine 72, sp. gr. 1014, solids 1008, urea (Haughton) 489. 9th. Weight 150 lbs., diet full—4 oz. extra bread, b. and s. = 3.41, 2.54, 10.88 = 16.83. 12th. Urine 82, 1011, solids 892, urea (Haughton) 434. 17th. Weight 153 lbs.

The first diet was evidently quite insufficient for this man, even while in bed; though he was not suffering much, or otherwise showing signs of requiring much food. The second was not quite



sufficient; the third was rather more than was necessary, but not more than was desirable.

Lewis, about forty, very small man; recovering from severe bronchitis. February 18th. Weight  $107\frac{1}{2}$  lbs., diet middle,  $\frac{1}{2}$  pint beef-tea, 2 oz. brandy, b. and s. =  $3\cdot02$ ,  $2\cdot25$ ,  $9\cdot64=14\cdot91$ . 27th. Weight  $108\frac{1}{2}$ ,  $\frac{3}{4}$  ss., cod-oil added, and 1 oz. brandy taken away,  $3\cdot02$ ,  $2\cdot75$ ,  $9\cdot64=15\cdot41$ . March 9th. Weight 115 lbs.; gain  $6\frac{1}{2}$  lbs. in 11 days.

Sheppard, thirty-six, excavator, 5 feet 2 inches, average weight about 120 lbs.; recovering from sciatica; an epileptic. Treatment, warm baths, hypo-phosphite of soda. No fits while under treatment. February 18th. Weight 117 lbs., diet middle—b. and s. =  $2\cdot73$ ,  $2\cdot25$ ,  $9\cdot64=14\cdot62$ . March 6th. Weight  $119\frac{1}{4}$  lbs. Here the diet was, probably, exactly right.

Burris, thirty-five, forgerman, height 5 feet 4 inches; strong muscular man, with some remaining hemiplegia from an apoplectic attack two years before. March 25th. Weight 152 lbs., diet middle—with 1 oz. sugar, no tea =  $2\cdot71$ ,  $1\cdot72$ ,  $9\cdot65=14\cdot08$ ; 12 grains iod. pot. daily. 30th. Urine of 4 days' average, 37 oz., sp. gr. 1025, solids 925 grains, urea (H) 510 April 3rd. Weight  $152\frac{1}{4}$  lbs., 6 grains iod. pot., 30 grains hypophosphite of soda daily. 10th. Weight 152 lbs., urine 2 days' average,  $29\cdot5$ , sp. gr. 1027, solids 796, U(H) 411; hypophosphite of soda with strychnia. 15th. Urine 2 days' average, 39, 1021, solids 819, urea (H) 486; constipated. 16th. Weight  $151\frac{1}{2}$  lbs. 24th. Weight 150 lbs.,  $\frac{1}{2}$  pint milk added, diet  $3\cdot11$ ,  $2\cdot07$ ,  $10\cdot10=15\cdot28$ . May 1st. Weight  $150\frac{1}{2}$  lbs.,  $\frac{1}{2}$  pint more milk, diet  $3\cdot51$ ,  $2\cdot42$ ,  $10\cdot55=16\cdot48$ . 6th. Urine 2 days' average, 44, 1013, solids 572, U(H) 286. 8th. Weight  $152\frac{1}{4}$  lbs. 15th. Weight  $151\frac{1}{2}$  lbs. 17th. Urine 36 oz, 1028, solids 1008, urea (H), 504. 22nd. Weight  $151\frac{1}{2}$  lbs., urine 2 days' average,  $28\frac{1}{2}$ , 1027, solids 768, urea (H) 397; had been purged by pills. The diet appeared now to be sufficient, but as the man was much below his former healthy weight I increased it again, and added 1 egg, 4 oz. bread, diet  $4\cdot10$ ,  $2\cdot66$ ,  $12\cdot63=19\cdot39$ . 23rd.  $\frac{1}{2}$  pint beef-tea,  $4\cdot40$ ,  $2\cdot66$ ,  $12\cdot63=19\cdot69$ . 29th. Weight 155 lbs.;  $3\frac{1}{2}$  lbs. increase in 7 days. June 1st. Average of 3 day's urine,  $28\cdot6$ ,  $1025\frac{1}{2}$ , solids 731, U(H) 394. June 3rd. Weight 156 lbs., 4 oz. bread struck off, diet  $4\cdot06$ ,  $2\cdot62$ ,  $10\cdot55=17\cdot23$ ; added 6 grains iod. pot. daily to medicine. 8th. Weight  $155\frac{1}{2}$  lbs. 10th. Urine 2 day's average, 24 oz., 1026, solids 624, urea (H) 333. 12th. Weight  $158\frac{1}{2}$  lbs. 19th. Weight 156 lbs.

Richards, coachman, twenty-six, height about 5 feet 6 inches; advanced phthisis; quick pulse. May 17th. Weight  $124\frac{1}{2}$  lbs., diet middle— $\frac{1}{2}$  pint milk, butter and sugar, cod-oil  $\text{3ii.} = 3\cdot13, 2\cdot90, 10\cdot10 = 15\cdot88$ ; steady improvement in symptoms from second day. 30th. Weight 122 lbs., diet as above, with 2 oz. meat for breakfast,  $\frac{1}{2}$  pint more milk, and  $\frac{1}{2}$  pint beer— $4\cdot03, 3\cdot65, 11\cdot15 = 18\cdot83$ . June 5th. Weight 124 lbs.

Patch, forty-five, convalescent from rheumatic fever, and walking about the ward; a tall, gaunt man; taking quinine, gentian, and iodine of potassium in small doses. February 27th. Weight 158 lbs., diet middle—beef-tea  $\frac{1}{2}$  pint, milk 1 pint, b. and s.  $= 3\cdot83, 2\cdot96, 10\cdot54 = 17\cdot33$ . March 1st. Urine, 2 days' average, 66, 1017, solids 1122, U(H) 528. 2nd. Weight 162 lbs., diet as above, with bread 4 oz.  $= 4\cdot27, 3, 12\cdot62 = 19\cdot89$ . 4th. Urine, 2 days' average, 78, 1017, solids, 1326, U(H) 624. 6th. Weight  $162\frac{1}{4}$  lbs. The additional bread seems to have been worse than useless, the whole of its nitrogen having gone to augment the urea.<sup>a</sup>

Brown, seaman, thirty-two, middle stature; melancholico-phlegmatic temperament; dormant phthisis; slow pulse; taking iron and quinine. January 30th. Weight 133 lbs., diet middle—bread 6 oz., beef-tea 1 pint, b. and s.  $= 3\cdot84, 2\cdot33, 12\cdot76 = 18\cdot93$ . February 9th. Weight 138 lbs.; 5 lbs. in 10 days, a considerable increase on a moderate diet.

Hale, thirty, painter, small man; convalescent from painter's cholic; taking 6 grains of KI per diem, with gentian. October 17th. Weight 106 lbs., diet middle, not much b. and s.  $= \text{say } 2\cdot71, 1\cdot70, 9\cdot35 = 13\cdot76$ . 21st. Urine 48, 1012, solids 576, U(H) 283. 22nd. Urine 52, 1014, solids 728, U(H) 353. 24th. Weight  $105\frac{1}{2}$  lbs.; went out. November 7th. Weight  $112\frac{1}{2}$  lbs.; had returned to hospital to be weighed; his clothes were nearly the same. In this case the poor man had not got, while in the infirmary, a sufficient diet, as no other reason, that I could make out, would account for his subsequent rapid increase.

Jolly, seventeen, well-grown boy; recovering from second attack of acute rheumatism; quick excitable pulse; supposed to have had pericarditis in first attack; quinine; walking about the ward. January 14th. Weight  $119\frac{1}{2}$  lbs., diet middle, milk 1 pint, beef-tea  $\frac{1}{2}$  pint, b. and s.  $= 3\cdot83, 3, 10\cdot54 = 17\cdot37$ . 16th. Urine 2 days'

<sup>a</sup> Roughly, 3 parts of nitrogenous food produce 1 part of urea. For albumen is to its contained nitrogen as  $6\cdot33$  to 1; and urea to its nitrogen as  $2\cdot14$  to 1; and  $2\cdot14 : 6\cdot33$  very nearly as 1 to 3. The rule is not new, but is not generally known.

average, 54, 1021, solids 1134, urea (H) 673, weight 121 lbs. 19th. Urine of 2 days' average, 62, 1019, solids 1178, urea (H) 719, weight  $122\frac{3}{4}$ ; went out. February 2nd. Returned to weigh, weight 125 lbs. It is difficult to understand how this lad could have been increasing in weight, and at the same time passing so large a quantity of solids. There was some reason for suspecting an error (but not a large one) in the specific gravity.

A slight attack of pneumonia, and speedy convalescence, afforded me an opportunity of experimenting on myself. I had, however, no weighing machine at hand. I found that with a diet of about 5· nitrogenised, 3·75 fatty, and 11· amylo-saccharine, or nearly 20· altogether, consisting mainly of eggs, milk, &c., I was passing as little as 41 oz. of urine daily, of sp. gr. 1020, yielding by estimate 820 grains of solids.

Dr. Chambers quotes the case of a patient of his, a girl, who had gained 4 lbs. in six days, "on the sole allowance of 3 pints of milk, and less than 12 oz. of bread, per diem." This is a good rate of increase, but the diet stated would yield 3·42 N, 2·22 F, 8·94 Am, = 14·58, which would be a large diet for many girls. The nutritive powers of milk are generally under-estimated. The milk diet of an hospital is generally supposed to be much inferior in this respect to the meat diets, but this is not always the case, as may be seen by a reference to the foregoing tables.

It is obvious that these experiments ought to be much extended before one could draw any very positive conclusions from them; and, moreover, that if sufficiently followed up and made more precise they might be applied to the determination of the necessary quantities of food for the support of the bodily functions, with much more probability of success than the ordinary plan of calculating the excretions alone, which has been already practised to a great extent.

At present, all I wish to base upon them, is the opinion, that a convalescent hospital patient may often consume a very large quantity of food, without any, or without a commensurate increase of the urinary solids, but with considerable gain of flesh. In such a case I maintain that it is true economy to add largely to the ordinary diet, for by so doing the patient's sojourn in hospital may be considerably abridged, his power to work may be sooner restored to himself and the public, his bed will be sooner available, and the finances of the hospital will not, *quoad* the individual patient at least, be at all more drawn upon. I by no means intend to affirm,



however, that for the ordinary run of cases, where patients are not much emaciated, or reduced in muscular power, the diets usually employed, *e.g.* the middle diet of the Bristol Infirmary, are at all deficient in quantity: on the contrary I have found that patients of that class, especially men of small frame, appear often to thrive upon them. The estimate of what Dr. Playfair calls a "subsistence diet," for a man without exercise, which he makes in his valuable lecture "On the Food of Man, in Relation to his Useful Work" (published since this paper was written), is to some extent confirmed by this last observation, and Dr. Inglis's testimony to a similar effect, is of value, as the diets at Worcester Infirmary approximate very closely to Playfair's standard. Returning to the subject of convalescents, I would again draw attention to the very large proportion of their food which, when they are liberally dieted, is stored up by them in increase of bodily weight. Pigs, according to Lawes and Gilbert, appropriate far more (nearly double) from their food than sheep and oxen; and they seem to suppose this difference to depend on the greater digestibility of their food, as much, or more than an inherent difference of constitution and digestive power, so far as the two causes are separable. Now a fattening pig, they say, will consume daily about 4 per cent. on his live weight, of the dry substance of his food, and will yield 1 part of increase of live weight for nearly 5 parts of that dry substance: *i.e.*, a pig weighing 100 will consume 4, and add 0.8 to his weight, in one day. Only one of my men, Powell, added to his weight at a greater rate than this; but on the other hand, neither he nor any one of the others consumed more than  $1\frac{1}{2}$  per cent. of his weight daily, in dry aliment; so that the human convalescent gave a vastly greater return for his food than the fattening pig, a fact which might have been of great interest in the Fiji islands in the last generation, as proving the economic superiority of the "long pig." I attribute this difference, in a small degree only, to the superior character of the food of the men (for milk and meal will fatten men as well as swine), but chiefly to two causes, *viz.*, the peculiar activity of the assimilative organs induced by the wasting and semi-starvation of an acute illness overpast, and the different chemical constitution of the increase in the two cases. That of the fattening pig consists mainly (65 or 70 per cent.) of dry fat, while in the convalescent man it doubtless comprises much muscular tissue, blood, &c., which include a very large proportion of water. It is



only on this last supposition that the rapid increase in such cases as those of Powell and Williams can be accounted for.

One point of some practical importance still remains to be noticed; I mean the necessity, which indeed is generally recognized, of allowing a very copious diet to consumptive patients. The cases of M'Carthy and Richards exemplify this pretty clearly. Phthisicals are with difficulty admitted into many hospitals; but where they are so, it is as foolish as it is unjustifiable to withhold from them as much nourishing food as they can digest.

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ART. V.—*Ophthalmoscopic Notes.* By HENRY WILSON, F.R.C.S., &c., Assistant-Surgeon to St. Mark's Ophthalmic Hospital.

#### RETINITIS PIGMENTOSA.

AMONG the manifold discoveries made in the living eye since the introduction of the ophthalmoscope into practice, that of pigmentation of the retina, made by Von Graefe some years ago, is one of the most interesting. It is, no doubt, true that pigment was found and described in the retina by Von Ammon, Walther, and the now venerable Mackenzie, long before the ophthalmoscope was invented, but only in morbid specimens, in which it appears to have been regarded as a mere accidental peculiarity. Although, however, they were unaware of the true nature of the phenomenon, or of its occurrence as a disease, yet I venture to believe that the earliest recorded instances of retinitis pigmentosa are to be found in the writings of Walther and Mackenzie, the latter of whom relates, as an instance of congenital night-blindness, the case of a deaf and dumb boy, which, from its history, symptoms, and *post mortem* appearance, was, I have no doubt, an example of congenital retinitis pigmentosa.

Pigmental degeneration of the retina, styled by the French, *rétinite tigrée*, originally termed *morbus Arianus*, after a nobleman in whom the disease was remarkably developed, and now universally known as *retinitis pigmentosa*, consists, as its name denotes, in a deposition of pigment in, and degeneration of, the structure of the retina. Strictly speaking, however, the latter name is, I think, not always accurately correct, for it implies and conveys the idea of an isolated, independent, and active disease in the retina, whereas it usually occurs as the result of inflammatory

processes elsewhere, and the retina may be merely passively and secondarily engaged, as I shall mention presently. Independent development of pigment may and does undoubtedly occur in the retina without any other structure being implicated; but this appears to me to be the exception.

Eyes affected with this malady present externally but little peculiarity, except in the very advanced stage of the disease, when the pupil becomes very small, and responds but slightly to the stimulus of light, or in children when nystagmus occurs—an oscillating or to-and-fro movement, common to all congenital or early acquired ocular defects. One of the earliest symptoms generally complained of is night-blindness, at first partial, and in the advanced stage complete. Vision then begins to fail by day, and in such a manner that the field of vision is narrowed. There is, also, loss of the power of fixation; the ability of at once and promptly discerning any given object is diminished, and the patient sees best in strong light. There is no pain, nor is there—externally at least—any trace of inflammatory action. The disease is an exceedingly slow and insidious one, and terminates, as a rule, in blindness. It is essentially progressive, but continues sometimes as long as twenty years before terminating in complete blindness. It occurs congenitally, is hereditary, and is frequently complicated with opacities of the lens and in the vitreous humour. The ophthalmoscopic examination reveals a very marked contrast to the normal state; the fundus of the eye, instead of being of a uniform brilliant or dull red (according to the pigmentation of the choroid) presents patches or spots of a jet black colour, varying in shape and size, scattered irregularly over it. These spots are generally stellate, sometimes small isolated black specks, irregular in outline, having more or less numerous projecting processes, from which appearance they have been likened to bone corpuscles; or they occur as jagged irregularly shaped striæ; or grouped and massed together, forming a black irregular network. The black deposit is nearly always in greatest quantity at the place of division of the retinal vessels. These latter are, as a rule, extensively implicated in the disease, and present here and there streaks of pigment along their course, while at other places they are wholly or partially obliterated, or appear as very fine thin threads, which condition is usually best seen on the optic papilla. The optic nerve is generally somewhat atrophied, and has occasionally some isolated specks of pigment on its intraocular surface. Liebreich has observed a unique case of

pigmentation of the entire papilla, in which the deposit was confined to the optic disc, without any trace of it elsewhere in the fundus; it was, however, impossible to decide whether this pigmentation was a congenital anatomical peculiarity or of a pathological nature. Pigmentation of the retina commences usually at the peripheral and equatorial parts of the retina, and advances slowly, but surely, towards the optic disc.

The black deposit cannot well be confounded with accumulations of pigment which are sometimes seen in the choroid; that it is situated in the retina is ophthalmoscopically recognizable, from the circumstance of its being either in the same plane as the retinal vessels, or even anterior to these, so that they are concealed by it, as well as from the fact of the choroid being generally distinctly visible behind it. In the large majority of cases the choroid itself is extensively diseased, and its pigment epithelium so highly atrophied that the large choroidal vessels are clearly visible. In these cases the black specks, and masses of pigment in the retina, form a very marked contrast to the bright yellowish or red colour of the underlying choroidal vessels, and the dark red of the choroidal intervacular spaces. This condition can be very inadequately represented in anything but a coloured plate. I have, however, attempted to delineate it in the accompanying wood-cut, admirably executed by Mr. Hanlon, from a drawing I made of the fundus of the eye of a man aged 55, who stated that he enjoyed excellent vision up to the year 1862, when he noticed that he could not see as clearly as usual in the twilight. His vision has gradually decreased since then, both quantitatively and qualitatively, so that when he came under my care in Spring he could only read, and with very great difficulty, "canon" type (No. 18, Jæger). His field of vision was small, and it took him a considerable time to make out the exact outline of an object. He saw best in a bright light; the head was held erect, and the eyes appeared as if seeking the light—just the reverse of the condition seen in persons affected with uncomplicated cataract. About the time he noticed his vision becoming impaired he had suffered great domestic affliction and much anxiety. At this time, too, he had a feeling of great heat, and tightness in the head, and would sometimes stagger in the streets like a drunken person—symptoms probably referable to intracranial disturbance. The pupils were normally dilated, but sluggish; the lenses slightly muddy, the left having a few striæ at its circumference; the vitreous was clear and free from muscæ.



The ophthalmoscopic examination revealed the condition I have endeavoured to illustrate by the subjoined wood-cut, which represents the enlarged reversed image of the left eye.



The background of the image is formed by an unequally coloured structure, composed of bright yellow and red tortuosities, which vary in size, and possess well-defined outlines, with dark coloured island-like interspaces indicated in the cut by the white lines and shaded interspaces, which give but a faint idea of the beautiful yellow plexuses formed by the choroidal vessels, which have become visible in consequence of complete and extensive atrophy of the layer of choroidal epithelium. The colour is much lighter at the position of the yellow spot—to the left of the central disc in the cut—where the atrophy has not only destroyed the pigment epithelial layer, but has engaged the stroma of the choroid. In front of this background, that is nearer to the observer, a quantity of jet black patches and specks are scattered irregularly over the fundus; these are aggregated here and there into net-like masses, especially on the outer or temporal side; one large triangular-shaped mass is seen encroaching on the upper border of the optic papilla, and three smaller ones are seen on its surface, towards the temporal side. The retina appears very slightly hazy on the inner side, and around the papilla; the papilla itself is somewhat atrophied, of a greyish white colour; its outline is slightly indistinct and irregular, indented here and there. The condition of the retinal vessels is very peculiar; above and below a large dark-coloured gorged vessel arrives from the periphery at the optic disc, after entering which it is seen to become much smaller, to assume, apparently, a different direction from that which it previously possessed, and to taper gradually to a point—an appearance somewhat similar to the



condition known as excavation of the optic nerve. Besides these there are several small arteries, some of which are so minute and attenuated that they can only be seen by using a high magnifying power; some are altogether obliterated or disappear in the network of pigment. Pigment is seen along the coats of some of the vessels. The pigment is in the same plane or in front of the retinal vessels, and the denuded large choroidal vessels are distinctly visible behind it.

Opinions are still divided as to the origin of this very peculiar disease. Donders, who was the first to institute a careful microscopical examination of the structures engaged in it, considers it to be a special and independent affection, combined with, or due to, a chronic inflammatory process in the retina; his view as to the idiopathic development of pigment in the retina has been confirmed by a case in which Schweigger, of Berlin, found on dissection the choroid perfectly healthy and normal in its entire extent, while the pigment occupied a zone of the retina lying between the equator and the ora serrata, and was confined to the vessels. Other investigators have arrived at the conclusion that the pigment is derived from extra retinal sources; thus Schweigger, who has devoted considerable attention to the pathology of ophthalmic diseases, and who has published the best and most practical work on the use of the ophthalmoscope, maintains that in the majority of cases pigmentation of the retina is attributable to choroiditis, with effusion and secondary infiltration of the retina. A very strong argument in favour of this view is afforded by the observations made independently by Schweigger and H. Müller, that at these places where the retina was unaffected and perfectly healthy, the subjacent choroid was likewise perfectly normal and unaltered, while at these spots where the retina was implicated in the disease, the choroid was correspondingly altered, and was devoid of its pigment cells. Some ophthalmologists still regard pigmentation of the retina as a mere accidental circumstance, which may ensue on any form of retinitis. In the case I have cited it would seem from the condition of the vessels that there had been an independent and idiopathic development of pigment in the retina, while the present condition of the choroid points unmistakably to the existence of choroiditis, so that the pigmentary deposit may be attributed to both causes—intra and extra retinal.

Our knowledge of the causes and mode of origin of pigment as a pathological product is, I think, still a limited one. Melanotic

formations occur in various tissues and secretions, as in pleural adhesions and in sputa; the small cerebral arteries are also occasionally found to have undergone pigmental degeneration. Paget states, with reference to this subject, that the general tendency of inflammatory products is to imitate the properties of the natural products of the part inflamed; and so far as disease of the lungs, choroid, and iris are concerned, this explanation is verified; but whether it will be found satisfactory in all cases of pigmental degeneration is doubtful.

The pigment is found, on microscopical examination, to be principally in the layer of grey nervous matter, and on the vessels. It occurs, however, in the various layers of the retina. It is found as pigment granules, or as pigmentated cells, which cells are flattened, polygonal, discoid, or elongated, the larger polygonal ones having a bright nucleus. One or more of the retinal laminae is, as a rule, atrophied. From its proximity to the choroid, the bacillary (Jacob's) layer is the most frequent to suffer, and is generally highly atrophic. The walls of the vessels are found to have undergone a hyaline thickening whereby the tube is narrowed or even obliterated.

A remarkable feature in this disease is its hereditary nature, and its occurrence in the children of consanguineous parents. In thirty-five cases, which Liebreich noted more particularly, he was able to prove descent from blood relations in nearly half. Thus, another example has been added to the list of ocular and other defects dependent on consanguinity, to which Sir William Wilde has drawn attention in his writings, more especially in his essays on Deaf Dumbness and on Congenital Malformations of the Eye, published many years ago in this Journal.

I know of no special or general plan of treatment which will affect this disease permanently; attention to and improvement of the general health, and alteratives will, at first, usually produce a marked change for the better; but this beneficial result is merely temporary and fleeting, and after some time vision will return to its former condition, and the disease will advance steadily, until eventually the patient is hopelessly blind. Opacities of the lens and in the vitreous humour frequently supervene.

Illustrations of this affection are given in Plate 6 of Liebreich's magnificent *Atlas der Ophthalmoscopie*, in Plates 6 and 8 of Jæger's *Beiträge zur Pathologie des Auges*, in Plate 79 of Sichel's *Iconographie Ophthalmologique*, in Plate 19, Figs. 9 and 10 of Ammon's

*Klinische Darstellungen der Krankheiten des Menschlichen Auges, in Wedl's Atlas der Pathologischen Histologie des Auges, &c.*

#### DISLOCATION OF THE LENS.

Luxations of the transparent or opaque lens, some varieties of which I described in the *Ophthalmic Hospital Reports*, are not of uncommon occurrence, and have probably been seen by the majority of practitioners. I will, therefore, now merely mention a peculiar phenomenon which is sometimes noticed during the ophthalmoscopic examination of such cases, and which may not be generally known. In certain abnormal positions of the transparent lens the observer is startled by seeing *two* optic nerve entrances instead of one. Some time ago a patient came under my care with hypermetropia of the right eye, and dislocation of the left lens. This person had received an injury of the left eye four months previously, and I found the lens dislocated inwards, in the vitreous chamber, with its equator nearly in the centre of the pupillary space, and still sufficiently transparent to allow of a good view of the fundus of the eye. The internal structures were considerably altered, the retina was hazy, and its vessels indistinct; the optic papilla was ill defined in every respect, but still visible. In the reversed image two optic discs were visible, one to the temporal side and the other to the nasal side of the fundus. These could be made to coalesce almost entirely, immediately behind the equator, by lateral movements of the convex object lens. The cause of this phenomenon was that one image was projected by the lens and refracting media, the other, on the contrary, by the refracting media alone, without the assistance of the lens; and the dislocated lens caused the image projected by itself to occupy a different position from the one in the production of which it had no participation.

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ART. VI—*Case of Fracture of the Spine, in which the Operation of Trephining was Performed, with Observations.* By ROBERT M'DONNELL, M.D., F.R.S.; Fellow of the Royal College of Surgeons; Surgeon to Jervis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University.

UNSUCCESSFUL cases are often more instructive to the practical surgeon than those which terminate favourably; with reference to the subject which I propose to discuss in the following pages this is

strikingly true, for I cannot help believing that thoughtful persons will be led to look favourably upon the operation of trephining the spine in certain cases of injury, as much from a careful consideration of some of the cases in which this operation has failed to save life, as from the account of those in which life has been actually saved by it.

It must be admitted that the question as to the judiciousness of trephining the spine, with a view to remove pressure from the spinal marrow, is one of the nicest which can be presented to the surgeon; and we know that surgeons of the greatest eminence are to be found ranged on both sides of the controversy—a controversy which, I regret to say, has not always been carried on with the calmness which should characterize such debates.

Before myself determining to recommend this operation, or to perform it if a suitable case should occur in my practice, I had very carefully studied what has been written both for and against it, and, so far as was possible, I had examined accurately the details of the published cases in which the operation had been performed. After due deliberation I had come to the conclusion that even on *purely theoretic* grounds, and viewed by the light which modern physiology and physiological experiment throws upon the subject, the weight of evidence lay on the side of those who, with Sir Astley Cooper at their head, advocate the operation. But in the advancement of surgery everything is essentially tentative, and results of cases must be the true criteria by which the most plausible theories are to be tested. An analysis of these results appears to me to lead irresistibly to the conclusion that in many instances this operation is good surgery; but in attempting to form a just appreciation of the value of the results of these cases, I do not intend to refer simply to those in which life seems to have been saved by the operative procedure; in not a few of the cases, which ultimately ran a fatal course the improvement which followed the operation was at least sufficient to show that the removal of pressure from the spinal cord is the best method which can be adopted in order to try to break the chain of sequences which, when uninterrupted, leads the patient suffering from fractured spine so inevitably to the grave. The case which I have to detail belongs to this latter group:—

Joseph Collins, aged thirty-eight years, a thin, spare man, ordinarily employed at labour, was admitted to Jervis-street Hospital on December 28th, 1864. A short time before, while



working in the hold of a vessel, from which a cargo of wheat was being discharged, a sack of corn had fallen upon him from a height of seventeen or eighteen feet. The weight fell upon the back of his head, neck, and shoulders; he sank beneath it, and to use his own expression, was "doubled up." Immediately after the injury his lower limbs were powerless; he was at once conveyed to hospital, where I happened to be at the moment of his admission; I had, in consequence, an opportunity of examining his spine at the seat of injury before any tumefaction had occurred. I found that the spinous process of a vertebra, corresponding with either the first lumbar or the last dorsal, was more prominent than natural, while a marked depression, leaving no doubt whatever as to the existence of displacement between the vertebræ, was to be felt in the place of the spinous process of the vertebra above. When a circle was made round the body, at the level of the umbilicus, with a piece of cord, the prominent spinous process was found to be exactly four inches above this circle; it was accordingly fixed upon as being the spinous process of the first lumbar vertebra. The lower limbs were paralysed, as were also the bladder and rectum. The urine drawn off by catheter was not bloody. At a consultation on the afternoon of the day of the patient's admission, and again the following morning, I urged as strongly as possible the necessity for operation. I could not, however, induce the majority of my colleagues to take my view of the case.

It is hardly necessary to detail the progress of the case during the days which immediately followed; the loss of power of motion and of feeling became more marked than just after the injury; the urine dribbled away; the feces were passed involuntarily; on the ninth day the urine was neutral; on the eleventh alkaline, and containing copious muco-purulent deposit; a few days later it became bloody, and of a most offensive odour. Although placed on a water bed, and attended to most carefully, bed sores formed; the penis became ulcerated, and the scrotum œdematous, and of great size. He suffered from thirst, flatulence, and pains over the bladder.

During the last days of January my friend, Dr. Brown-Séquard, happened to arrive in Dublin; he saw the patient with me, and thought that even still operation offered a chance for life, instead of certain death; the majority of my colleagues agreed, and with the patient's consent, indeed at his urgent request as soon as he understood the hopeless nature of his case, the operation was determined upon.

I now copy from my note book the details of the case from day to day.

*Condition immediately Previous to Operation.*—Dr. Brown-Séquard arrived in Dublin on January 30th; the following morning he accompanied me to visit the patient. He was then carefully examined, and was found in the following condition:—Pulse 100, regular, but feeble; tongue clean, rather dry; had rested tolerably well the previous night; no headache; complains of flatulence, and has a tendency to diarrhea; bladder completely paralysed; urine constantly dribbling away; no urine accumulates in the bladder; he is absolutely unconscious of the discharges passing “per anum;” and fluid feces ooze incessantly from the rectum; the penis is swelled, and the prepuce ulcerated from constant contact with the urine; there is an ulcer as large as sixpence at the root of the penis, in the fold between the penis and scrotum, and the whole scrotum is swelled, red, and superficially excoriated; back, over the sacrum, ulcerated to a considerable extent, but, except in one small part, not deeply; at this one part matter can be pressed out, showing that the ulceration extends to some depth under the skin. Upon the inner ankle of left foot, and the outer ankle of the right two smaller spots (dry and scabbed) exist, where ulceration has occurred apparently without pressure of any kind having taken place. Paralysis of motion is almost complete in both lower limbs; in this respect they are exactly alike. On doing his best to produce a movement in the legs, a slight motion is perceived in each groin; the muscles of the thigh above the knee, and of the calf, and of the toes are motionless; no reflex movement can be excited; sensation is normal along the thighs, over the skin of the calf and shin; rubbing or pressing on the sole of the foot is not perceived; he can, as regards the thigh and calf, distinguish the compass points at the usual distance, as in persons who feel normally; he can tell what part of the thigh or calf is touched by the hand, and in these parts distinguish, with precision, heat and cold, and also pricking with a point. There is no difference in these respects between one limb and the other; in each foot sensibility is much impaired, and the sole of each is devoid of feeling altogether.

*Operation.*—I operated on February 3rd, 1865. My colleagues, Drs. Hughes, Stapleton, Banon, Tyrrell, and Forrest were present. Dr. Brown-Séquard was also present, and many surgeons of Dublin.

The patient was not removed from the ward or from the bed on which he lay. This was in order, as far as possible, to avoid disturbing any callus which might possibly have already formed. He was put under the influence of chloroform as he lay on his back in his accustomed position; when chloroformed, the bed was carried opposite to the window. The patient was turned over on his face; in this position the seat of the injury was obvious from the prominence of the last (?) dorsal vertebra. A small spot of ulceration, not so large as a sixpence, existed over the most prominent spinous process at this part.

An incision, nearly five inches long, was made with a strong scalpel over the spinous processes of the vertebræ (the two last dorsal and two first lumbar); then, with a strong curved bistoury, the slips of tendon on each side of the spines were divided. Keeping quite close to the bone the mass of muscle was detached on each side, and securely held apart by broad retractors, well suited for the purpose. The spinous processes, and laminae, in fact, the back of each vertebra as far out as the articulating process was thus fairly and fully exposed. The spine of each exposed vertebra was then taken hold of in a strong pair of necrosis forceps, and cautiously but firmly shaken to try whether any fracture of the posterior arch, or of the processes could be detected. No such fracture existed. (This I expected, as I had reason to suppose from the nature of the injury, that if any fracture existed it was one of the body of the vertebra).

On examination I was satisfied of the displacement existing in the parts with which I had to do; it was as follows:—The last dorsal vertebra (?) was as it were twisted, so that on the left side the articulating process was raised up, and although not completely dislocated, yet it stood prominently backward from the corresponding process on the bone below; on the right side the superior articulating process of the same vertebra was in exactly the opposite predicament; it was displaced so as to be as if pushed in deeper than the process to which it corresponded on the vertebra above.

I determined to remove the spinous and inferior articulating processes of this vertebra, by cutting through the laminae. After waiting for a time while sponges and cold water were applied to stop bleeding, I first took off a part of the spinous process of the part I was going to remove, and divided the interspinous ligaments above and below. I next, with a strong pair of bone forceps, cut through the lamina on the patients left side (on which side I stood

myself). This, owing to the displacement I have already described, was quite easily effected, as, of course the lamina, as well as the articulating process, was raised up somewhat on this side. On the right side, however, the opposite state of things made it much more difficult to succeed in cutting through the lamina. I could not succeed at first with the cutting forceps, but was obliged to use a Hey's saw guarded, so that it could not go in beyond a certain depth.

I was unwilling to use the saw, lest the unavoidable shaking might injure any callus already thrown out, and tend to undo any repair that nature had already commenced.

I finally succeeded in dividing the lamina on the right side with the forceps, I then grasped, in a pair of necrosis forceps, the root of the spinous process of the portion thus separated, and raising it cautiously, divided, with a scalpel, the ligamentous structures which now alone prevented its removal. On sponging away the blood, the theca vertebralis came into view; a small portion of the arch of the vertebra above was also removed with gouging forceps. The spinal cord was obviously pushed backwards, and had lain very close under the arch of bone taken away. The vertebral theca was not tense; there was no evidence of either blood clot or fluid being pent up within it; it was therefore not opened, although I had at first intended to do so.

Slight venous hemorrhage took place from the veins underneath the bone.

The operation lasted about an hour; it was protracted by long rests now and then, waiting until the sponging with cold water and infusion of matico checked oozing of venous blood, as it was necessary to see with great precision what one was doing. I do not think that more than five, or at the most six, ounces of blood was lost. A couple of sutures were put in at the upper part of the wound; the lower part was left open, a small tent of soft sponge alone being introduced.

*Notes immediately Subsequent to Operation.*—The patient was replaced upon his back, pads of spongio-piline being placed above and below the wound, and the sacrum protected by a circular cushion. He lay on a water bed, nearly flat, the head but slightly raised, and a cushion being passed behind the knees. He got an opiate enema; while being arranged a jet of urine was thrown from the orifice of the urethra; of this he was not conscious.



The same evening he commenced taking the 96th of a grain of atropine in solution, to be continued three times a day.

The bladder and the large intestine were carefully washed by injections of tepid water and thin flaxseed tea.

February 4th.—Day after operation; pulse 120, regular but weak; had rested tolerably, sleeping for a couple hours at a time, some headache, and heat of head and skin; tongue dry, but not coated. When about to wash out the bladder, the urine was observed to come in a jet from the orifice of the urethra; bowels had not acted since the previous day, the opiate enema having checked the diarrhea; the pupil was not affected; the atropia was continued in the same dose as before; the penis and scrotum diminished in size, and the superficial ulceration better. In the afternoon, Dr. Brown-Séquard accompanied me to see him; we then found that sensibility had returned in the soles of the feet, and that a decided return of motor power had taken place in the muscles of the thigh. Complaints of cough, which hurts him—in fact, he had caught cold from the exposure during the operation. Ordered iodidi potassii three grains, in decoct. cinchonæ flavæ, along with the atropia.

5th Feb.—Pulse 108. Skin warm, but natural; tongue moist; had rested tolerably; no headache; appetite returning; had an egg for breakfast, and asked for a mutton chop for dinner; cough much less troublesome. Dr. Brown-Séquard again this day examined him along with me; we observed some œdema of the left leg and foot; sensibility is now almost, if not quite, normal all over the foot and sole. The sartorius, hamstring, and quadriceps extensor femoris muscles are able to contract with considerable strength; we can perceive no sign of movement in the muscles of the calf, or in the toes; their motor power is still absolutely wanting. Marked improvement has taken place in the state of the penis and scrotum. I dressed the wound and the sore on the sacrum, causing the patient to be raised right up by five assistants; after the application of the dressing, he was replaced on the circular cushions as before; healthy pus coming from the sore over the sacrum. When he coughs he feels pains across the back. Bladder and rectum washed out as before.

6th.—Pulse 100. Tongue moist; skin natural; altogether appears to be going on satisfactorily; has had a free evacuation from the bowels of natural appearance and consistence. He states distinctly that although not able to control the passing of the evacuation, he was conscious of its passage.

The bed clothes underneath the patient were entirely changed; the wound and sore on sacrum dressed; the latter looking clean, and secreting healthy pus. Hitherto I should say that the most marked improvement which I can notice in the patient is that in the condition of the sores on the sacrum, and those on the prepuce, penis, and scrotum.

Atropia continued, with decoct. cinchonæ and ioidid. potassii. Eats an egg at breakfast, a chop, with porter, for dinner, and an egg in the afternoon. Bladder washed out several times a-day.

7th.—Appears to be making progress satisfactorily. I examined the condition of the lower limbs this morning, in presence of my colleagues, Drs. Lyons and Forrest; Dr. Beatty was also present, and the class. I think I may say that sensation is now normal everywhere; he can tell (without being allowed to look at the part touched) when the finger is gently applied or rubbed over any part, even of the sole; he distinguishes heat and cold in the sole as well as elsewhere. The motor power of the muscles of the thigh is much increased; the patella is moved with some force; as yet the muscles of the calf and toes are incapable of movement. Diet as before; atropia slightly increased; iodide of potassium and bark continued. Washing of the bladder continued several times daily.

8th.—Had a good night. Pulse 100; sores looking well; œdema of left foot nearly gone. Wound and sore on sacrum carefully dressed; bladder washed. Atropia has decided effect on the pupil. No observation made this day on sensation and motion of lower limbs.

9th.—Rested very well. Bowels freely moved without control, but with consciousness of the movement; penis and scrotum much reduced in size; œdema of left leg nearly gone. No particular examination of motion and sensibility made this day. Wound suppurating, and apparently going on favourably.

10th.—Had not a good night, yet does not seem worse as regards his general condition. Pulse 108; tongue clean and moist; bowels not moved during last twenty-four hours. For the first time to-day I observed distinct reflex movements of the muscles of the thigh, on touching or plucking the hairs on the inside of the thigh; in other respects as before. Atropia continued. Cough is rather troublesome; he attributes his restless night to this, not to pain; he has no referred sensations of pain in the lower limbs.

11th.—Pulse 108, and in other respects the same as yesterday; well-marked voluntary movements in all the muscles of the thigh;

none in the calf or muscles of the toes; no reflex movement in the calf or toes from touching or tickling the leg or sole. Reflex movements in the sartorius and adductors are to-day well marked, when the hairs on the inside of the thigh are plucked. Scrotum and cremaster also give reflex movements. The condition of the penis and scrotum much improved. The wound was dressed to-day; a copious discharge of pus, dark with blood. Bed sores on sacrum improved, and discharging healthy pus.

12th (Sunday).—After being dressed yesterday he had a severe rigor; this may be due to the almost unavoidable exposure to cold while the cloths underneath him are being changed. Bowels moved this morning; feces natural in appearance and consistence.

13th.—Pulse 100, feeble; sickness of stomach came on last night; is afraid to eat for fear of bringing on vomiting again. I injected the bladder to-day with turpentine  $\mathfrak{z}\text{i}$ , made into an emulsion, with yolk of egg; it produced no pain. Atropine is still continued; iodide of potassium stopped, as he fancies it causes sickness. Wound dressed to-day; edges very red.

14th.—Passed a good night; is altogether better to-day; pulse 100, and stronger; vomiting ceased; took his egg this morning.

15th.—Passed a quiet night; sleeps a good deal; bowels moved to-day; no material change since yesterday. The bladder is washed out daily with the turpentine emulsions, and I think I may say that the condition of the urine washed out indicates some improvement in the state of the bladder. The urine is certainly much less offensive, and the mucus, which was at first increased by the turpentine, is now diminished in quantity. Wound dressed; a large discharge of healthy matter; the bed sores are clean, and in a healing state.

16th.—To-day, for the first time, there is a marked improvement in the power of expelling the urine, on trying to do so.

17th.—Pulse 120, weak. Passed a quiet night, free from pains; he sleeps a good deal both by day and night; can now drive the water out of the bladder with a considerable degree of force. Motion, sensibility, and the reflex phenomena are in the same condition now for some days, neither getting better or worse. Scrotum quite pursed up, and of natural dimensions; penis parboiled, and swollen round the prepuce, but of natural size in other respects; bed sores going on well; wound secreting a large quantity of matter.

18th.—Had a rigor yesterday after being dressed; sickness has returned to-day; brandy and water freely given.

19th (Sunday).—Pulse 125, very weak. Felt very weak to-day when being dressed. Bowels moved; discharge thin, and indicating diarrhea; sickness continuing, not checked by ice, hydrocyanic acid, &c. Bladder has been washed daily with the turpentine emulsion as before. I believe the condition of the bladder to be certainly improved, and the voluntary power over it so likewise. He says he did not sleep at all last night, but does not know what kept him awake as he had no pain. The urine continued alkaline throughout.

He died rather suddenly on the morning of the 20th, having been seen by the resident pupil about an hour before, and reproved for smoking, which he had been found doing by the night nurse. He was then apparently quite himself, and not in pain.

*Post mortem* examination, made the same day (20th), in presence of the pupils. Body much wasted; no œdema.

*Head.*—Considerable subarachnoid effusion, also some clear serum in each lateral ventricle; brain structure and its membranes healthy.

*Chest.*—Lungs healthy, indeed remarkably so; no old adhesions; heart normal; no fluid in pericardium.

*Abdomen.*—Stomach and intestines healthy; no lodgement in any part of bowels; no ulceration in the rectum or other parts of the large intestine.

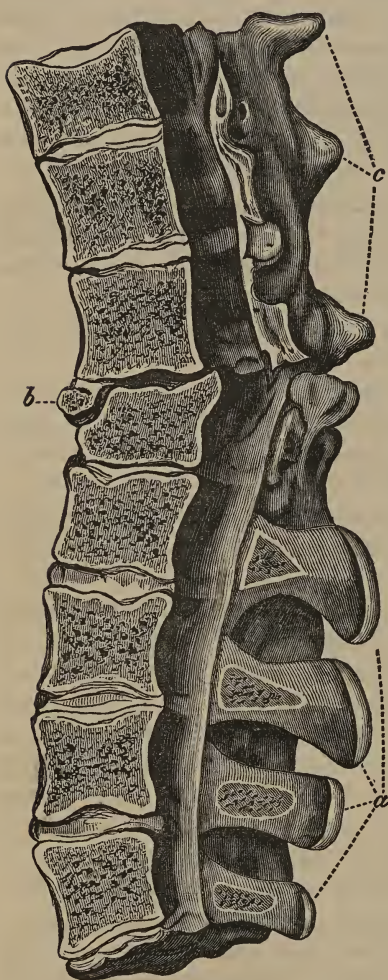
Urinary bladder contracted to a small size, greatly thickened, and containing small collections of pus in its walls. Mucous membrane ulcerated, and covered with ash-coloured shreds of adherent membrane; both ureters thickened, so as to be as thick as my little finger; the mucous membrane lining each was in the same condition as that of the bladder, and on the left side this extended all the way up to the kidney, the pelvis of which contained pus; its structure was disorganized by abscesses.

On examining the bodies of the vertebræ on their anterior aspect there was no inequality which made it obvious where the injury had taken place. The lumbar and lower dorsal vertebræ were removed; the spinal cord and its membranes were taken out by cutting through the laminae of the dorsal vertebræ. Subsequently the section shown in the accompanying woodcut (Fig. 1) was made. The dura mater of the cord was uninjured; the portion of it corresponding to the piece of bone removed at the time of the operation was covered externally with lymph; the surface next the cord was healthy; there was no trace of inflammation within the dura mater. The cord was not inflamed or softened; it was indented at a point



corresponding to where the bone was displaced; and when the finger

Fig. 1.



was passed along it gently it felt as if softened at this indentation, but there was neither red or white softening of its structure, and this feeling was merely the result of pressure, which had not given rise to structural disorganization.

The body of the first lumbar vertebra was fractured, and this vertebra was displaced backwards; the line of fracture separated only a small portion of the body of the broken vertebra. The intervertebral substance between the last dorsal and first lumbar vertebræ had been torn, and the body of the first lumbar was displaced backwards, as shown in the accompanying woodcut. A small blood-clot occupied the space above the projecting body of this vertebra, lying between the body of last dorsal vertebra and the anterior aspect of the dura mater of the cord. The spinal cord was, therefore, pushed backwards by this effused blood, as well as the body of first lumbar vertebra, not simply by the sharp ridge of bone, as might be at first supposed from looking at the woodcut.

Fig. 1 represents a vertical section of the inferior part of the spinal column, as seen in the case of Collins (the lumbar vertebræ and the three last dorsal): *a*, the spinous processes of the four last lumbar vertebræ; *b*, the small portion broken off from the body of first lumbar vertebra; *c*, portions of the last three ribs. As the cord was pushed backwards, not only by the projecting body of first lumbar vertebra, but by the blood-clot filling the hollow above this projection, it is obvious that the complete removal of the posterior arches of both the last dorsal and first lumbar vertebræ would have set the cord free from pressure.

It was, of course, corresponding with this part that the indentation existed upon the spinal marrow.<sup>a</sup>

As a sort of appendix, at the conclusion of this communication I have given a table of reference to all the cases which I have been able to find out in which an operation has been undertaken to remove portions of bone pressing on the spinal marrow. These amount to twenty-six cases, in seven of which life has been preserved, viz.:—No. 1, Louis'; No. 9, Alban Smith's; No. 13, Edwards'; No. 16, Walker's; No. 17, Blackman's; No. 23, Blair's; and No. 24, Potter's (twice operated upon). Although it must be admitted that the details of these cases are in some instances meagre and unsatisfactory, yet the fact of the cases having occurred can hardly be denied. I have already said, however, that the improvement which took place in some of the cases which ultimately terminated fatally is a result which shows not only that good is in many instances derived from the operation, but that some of the arguments used against an operative procedure are not well founded. In my own case this improvement was well marked; it was evidenced by return of sensation, increase of motor power, return of the possibility of exciting reflex movements, but, above all, by the altered condition of the penis, scrotum, and bed sores. Tyrrell says after the operation in his first case:—"The patient could now feel distinctly on being pinched on the inside of the thigh, which *immediate return of sensation was beyond my most sanguine expectation.*" Mr. Wickham tells us—"The operation was easily performed, and the patient was in a degree relieved by it; his breathing became more free, and *sensation returned to a considerable extent.*"

In Holscher's case, eight weeks after the injury, sensibility returned in the feet; some week's later movement of the legs was possible. In Jones' case the amendment was so remarkable that I shall be pardoned for transcribing the case in full:—

"J. G., aged 34, a tall and remarkably muscular man, by trade a carpenter, was brought into the hospital, at 3 P.M., on the 28th February, 1856, in consequence of an accident he met with three-quarters of an hour before his admission. While employed on the deck of his vessel, his foot slipped, and he was precipitated into the hold, his head, at the same time, coming in contact with a block ;

<sup>a</sup> These parts were exhibited at the meeting of the Pathological Society, February, 1865, and are preserved in the Museum of the Carmichael School of Medicine.

he was taken up in a perfect state of insensibility, and shortly afterwards was seen by Dr. Marett, who accompanied him to the Hospital, where he was immediately attended to. It was evident that he then laboured under the effects of liquor, the breathing was rather stertorous; pulse 38, soft and feeble; pupils natural; excessive priapism. With the exception of a slight abrasion over the right eyebrow, there existed no sign whatever of external injury; the extremities were cold, so that hot water bottles had to be applied to them. When seen at 9 p.m. he had entirely regained consciousness, and complained of pain in the neck, and great uneasiness between the shoulders; also slight pain in the head; had sensation along the arms, but not in the fingers; there was entire paralysis and loss of sensation from the sternum downwards; breathing was consequently performed by the diaphragm alone. Pulse 54; no irregularity in the line of spinous processes was perceptible; the head was thrown backwards, and the integuments at the back part of the neck presented three or four folds running transversely. A strong cathartic was at once ordered, and ʒxxvij. of healthy-looking urine was drawn off by means of the catheter. From this date up to the fifth evening after the accident no melioration took place; on the contrary, the patient slowly, but gradually, became worse; the catheter had to be introduced three and four times in the twenty-four hours, as all power of voiding urine was completely lost; the bowels were only acted on by means of croton oil; assisted by enemata; the pulse varied from 50 to 60; and, notwithstanding the precautions taken, a large sore on each of the nates had already commenced sloughing. On the 2nd March the sufferer was placed under the influence of chloroform, in order that a more minute examination of the spinal column might take place; the result was as unsatisfactory as the first. When seen on the 5th, at 9 a.m., a marked change for the worse had evidently taken place; a great tendency to lethargy, which had never before manifested itself, was apparent; and when roused, the countenance expressed considerable anxiety; the sensation along the arms was more deadened; speech thickened; and the pulse intermitted at every third and fourth beat. Under these most unfavourable circumstances, the following operation was performed, the patient having first been rendered insensible by means of chloroform:—

“ An incision, about four inches in length, was first made on each side of the spinous processes of the third and fourth dorsal vertebræ, and were then connected together in the form of the letter H, by a



transverse one. As much of the muscular structures, together with their integuments, as the nature of the parts would allow, were then reflected upwards and downwards. The muscular mass on each side of the spinous processes, and that covering the vertebral arches, had to be cleared away, in order to expose those parts; this accomplished, the arches of the third and fourth dorsal vertebræ were sawn through close to their pedicles, and their interspinal attachments being loosened, were removed, so that the cord, covered by its membranes, was exposed; but little effusion of blood was found here, and no fracture could be detected; the incisions were, therefore, prolonged upwards, and, on exposing the lamina of the fifth and sixth cervical vertebræ, much ecchymosis was discovered, and the fracture detected. The same proceeding was adopted over these vertebræ as in the first instance, and the cord and its membranes exposed; the arches were fractured across; the membranes were not lacerated, but roughened; and there was much blood effused between them and the vertebral canal. After the removal of the fractured portions, the lips of the wounds were approximated by means of adhesive plaster, and water dressing, &c., applied. The hemorrhage was very abundant, though not serious, and chiefly of a venous character. The pulse after the operation rose to 80, and no longer intermitted. At 8 p.m. the patient had entirely recovered from the effects of chloroform; merely complained of smarting pain in the neck and back; was perfectly cheerful, and *had recovered sensation as low down as the umbilicus. The arms, too, which before the operation could only be drawn upwards, were now raised at pleasure, and could, without inconvenience, be thrown across the chest.*

“6th.—Slept comfortably during some parts of the night; pulse 70, and regular; sensation, as low down as the umbilicus, natural; has on several occasions felt the desire to void water, and has asked for the urinal two or three times; the power is, however, still wanting, and the urine has been drawn off. (It may here be stated that, from the time of the accident till the patient's death, this secretion was always sufficient, natural in character, and never once admitted the slightest ammoniacal odour.) On the 7th and 8th the improvement was gradual; and, at 1 a.m., on the 9th, I had some reason to hope that recovery might take place. I found he had slept well for five hours. He expressed himself as refreshed, was very cheerful, desired food, and gave it as his opinion that the catheter would not again have to be employed. Pulse was then 73, soft, and regular. At 5, his bowels were freely acted on from croton-oil pills, taken



the preceding evening. His linen was at once changed by the nurses, when almost immediately afterwards coma came on, and he expired at 10 o'clock the same morning. As the patient's relations were anxious to remove the body as soon as possible, the *post mortem* examination was of necessity a partial one, and, consequently, by no means as satisfactory as could have been desired. It showed the spinal cord to be entire and unlacerated in its posterior aspect. To the left side, opposite the pedicles, and passing obliquely across, and corresponding to the seat of the fracture through the bodies of the fifth and sixth cervical vertebræ, the cord bore the appearance of injury. Much blood was found in the neighbourhood of the fractured parts, and so complete was the injury and displacement, that the bones were preserved in their natural position almost entirely by the integrity of the anterior common ligament. The roots of the spinal nerves on the left side were in a great measure severed from the cord. There was considerable effusion at the base of the skull, and the muscles and structures on the back were congested."

I conceive that the amendment which was observed to take place in the cases to which I have now referred, offers the best answer to what has been urged as a cogent reason for non-interference in cases of fracture of the spine, viz., that as, in the great majority of instances of this injury, the fracture traverses the body of the vertebra, a displaced portion of which causes a diminution in the size of the canal, and as this portion is not accessible to an operation, it is argued that such a proceeding can be of no avail.

To give Sir Benjamin Brodie's<sup>a</sup> own words, "The question respecting such an operation seems to me to lie in a very small compass; if the whole or nearly the whole of a vertebra be driven forwards, the depression of the posterior part of it will, of course, occasion a diminution of the size of the spinal canal; but the removal of any portion of the vertebra, which is accessible to an operation, will be of little avail, as the irregularity in the anterior part of the canal, made by the displacement of the body of the vertebra, must be the same after, as it was before the operation." Mr. Alexander Shaw puts very strongly the same argument,<sup>b</sup> "Specimens," he says, "preserved in museums, prove it to be a

<sup>a</sup> Med. Chirurg. Transactions. Vol. xx., p. 160.

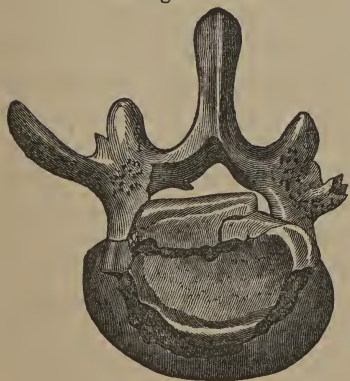
<sup>b</sup> A System of Surgery by Holmes. (Article—Injuries of the Back. By A. Shaw. Vol. ii., page 236.

general fact, that the fragments of the broken vertebræ, which are indented into the cord and commit the greatest injury, are in front, not behind. It does not appear, therefore, that benefit can possibly be gained, so long as the principal cause of compression continues to exist on the fore-part, by extracting portions of the posterior arches."

This line of reasoning has carried great weight with many; and I confess that it at first seemed to me the gravest of all objections offered against the operation in question.

The impartial enquirer will, I conceive, find the refutation of it simply in the facts shown in the cases already cited, which prove that benefit *has been gained* by extracting portions of the posterior arches; that motion and sensation have, more or less, completely returned, and ulcerations healed; in short, that the spinal cord has, to a certain extent, recovered its powers, and that, too, in cases in which the examination after death has shown the circumstances to be precisely those in which it would appear to Mr. Shaw that no benefit could possibly be gained. Sir B. Brodie's observation is, no doubt, quite true, that the irregularity in the anterior part of the canal, made by the displacement of the body of the vertebra, must be the same after as it was before the operation; but if the cord is pushed backwards, so as to be actually squeezed against the posterior arch, clearly the removal of the arch gives it room. Taking

Fig. 2.



away the spinous process and laminae in such a case as is figured in the accompanying woodcut (Fig. 2), would, without doubt, take pressure off the cord; and in my case the complete removal of two arches, as will be seen by looking at the woodcut (Fig. 1), would have set the cord completely free from pressure. The cord would, indeed, have still passed over an eminence, caused partly by bone, partly by the blood clot in front of its dura mater, but, the source

Fig. 2.—Showing a fracture of the body of a lumbar vertebra. The woodcut is copied from Dr. Gurlt's work on fractures (*Handbuch der Lehre von den Knochenbrüchen* von Dr. E. Gurlt), and is taken from a preparation belonging to Professor Middeldorp. It is obvious that in such a case, although pressure is caused by the fractured body, yet that it would be relieved by taking away the posterior arch.

of counter pressure being removed, we know very well that passing round an abnormal curve, even greater than this, does not prevent the spinal marrow from exercising its functions. I regret that in my case the posterior arch of the last dorsal vertebra was not taken away completely; it would have added little to the severity of the operation, and nothing to the danger, and would more thoroughly have taken pressure from the cord.

Although Boyer was opposed to the operation, yet one at least of the four cases which he adduces seems to give little support to the opinion which he forms.

“A sack of flour weighing three hundred pounds, fell on the nape of the patient’s neck; an acute pain was felt in the lower part of the cervical region. The patient was conveyed to the Hospital of La Charité; on examining him I perceived that the spinous process of the seventh cervical vertebra was more prominent than in the natural condition. The upper and lower limbs were paralyzed; respiration laborious; the rectum and bladder incapable of acting. The patient died at the end of five days. At the *post mortem* examination we found a fracture of the posterior arch of the seventh cervical vertebra, with depression of a fragment which pressed on the spinal marrow, and exercised upon it firm compression.”

The recorded cases of trephining of the spine show how groundless are the theoretic objections conjured up against this operation by Sir C. Bell; those persons, at least, who witnessed the operation in my case well know that he is quite in error in speaking of the great “degree of violence necessary for the performance of the operation,” and those pupils who saw the patient’s condition before and after the operation, and who watched his progress from day to day, will, I fancy, laugh at the ridiculous assertion that “the man must be already dead whose condition is not made worse by such an operation as this.” Sir Charles Bell asserts that even if a sharp piece of bone were driven into the spinal marrow, causing paralysis of the lower limbs, that exposing the medulla to extract the fragment would so aggravate the mischief that inflammation, suppuration, and death, would be the inevitable consequences. This assertion experience has contradicted, and as a matter of fact, we know that the operation is not likely to be followed by inflammation and suppuration of the cord or its membranes. Indeed, as regards the occurrence of meningitis, it is natural to suppose

that in trephining the spine this would be less likely to occur than in trephining the skull, as in the latter case, the dura mater is very closely adherent to the bone; while in the former it does not constitute a periosteal lining for the bone, which is without difficulty detached from it.

Liston also speaks of the chance of inflammatory action being increased by operation, to which he is decidedly opposed. Mr. Alexander Shaw draws a terrible picture of the inflammation and suppuration which he fancies is likely to follow:—"Let any one," he says, "consider the ulterior effects which may be reasonably expected to follow, in general, from such a wound as must be inflicted in trephining the spine. The incision must be both long and deep, being made through the skin and the muscles, and through the blood infiltrated in the torn structures, down to the broken bone. The main object of the proceeding being to remove the fragments, and to lay open the vertebral canal, the external wound will communicate not only with the fractured surfaces but with the interior of the canal which contains the spinal cord. To the mind of the surgeon accustomed to witness the disastrous effect of compound fractures, the condition here described must appear the most formidable that can be conceived. He will look forward (if the patient should live) to profuse suppuration being soon established; to the pus penetrating extensively along the vertebral canal, between it and the membranous theca, to the exposed and the isolated ends of the fractured vertebræ becoming necrosed, and he will not expect that, until these be detached and cast off (a process requiring many months for its completion), the suppuration will cease or the wound cicatrise."

The reply which I would give to all this is very brief; it is simply this: within the present year I have seen this operation twice performed in this city; once by myself, on a patient who survived the operation seventeen days, and once, six weeks ago, by my friend Dr. Gordon, on a case which is at the present moment going on favourably; and that in neither the one or the other of these cases were the terrible expectations of Mr. Alexander Shaw realized.

The wounds, of which one was about five, the other a little less than four inches long, suppurated, and granulated healthily, and were not sources of any considerable discomfort to the patients.

Meningitis, going on to suppuration, was not a cause of death in any of the recorded cases in which *post mortem* examinations were



made after the operation. In Oldknows' case, and indeed in Holscher's, the dura mater was thickened, and the pia mater of the cord vascular; in Meyer's case the spinal marrow was softened; as however, in my own limited experience, I have seen three patients, not operated on, carried off in great suffering by suppurative meningitis, the whole cord being bathed in pus; I must rather incline to the opinion that trephining tends more to prevent meningitis than to cause it.

Those surgeons who are opposed to the operation of trephining the spine are of course anxious to show that cases of fracture of the spine are not in all instances fatal; and it is obvious that before any surgeon would be willing to incur the weighty responsibility of recommending this operation to his patient, the very first thing he will do will be to inquire what may be the probable chances for life in cases left without operative interference. We have not, unfortunately, anything like accurate statistical information on the subject. It is well known, however, that recovery under such circumstances is extremely rare: as Jones observes, in common parlance, when it is said that a man has broken his back it is generally regarded as synonymous with his having met his death. Mr. Shaw mentions two cases of recovery: the first is that of a patient who recovered (with paralysis remaining) from fracture in the dorsal region, and who was seen by him in perfect health otherwise, more than twelve years after the accident. The second is that of a man who had fracture and paralysis in his youth, who shortly afterwards recovered sensation, and the power of motion in his limbs, so as to be able to follow an active occupation, who, after a period of seventeen years, had a return of the paraplegia, and lingering for five years in that condition, died under the observation of Mr. Shaw, who had the opportunity of examining the body.

He narrates also a very remarkable case communicated to him by Mr. Page, of Carlisle, in which a gentleman, twenty-six years of age, survived, for nearly fifteen months, an injury of the fifth (or fourth?) cervical vertebra, the body of which was broken, almost severing the spinal marrow.

Cases such as that detailed by Mr. Hilton, in which a patient with fracture of the cervical vertebrae, accompanied by complete paralysis of the arms, legs, bladder, and rectum survived for fourteen years; or Mr. Phillips'<sup>a</sup> well-known case of fracture of the

<sup>a</sup> Medico-Chirurgical Transactions. Vol. xx., p. 78.

atlas and axis, are looked upon with an amount of astonishment which is in itself the best proof of their scarcity.

Sir Astley Cooper alludes to two cases in which union by ossific matter had taken place; in one of these the patient died in twelve months, wanting nine days, after the injury, owing to a sore on the tuberosity of the ischium and disease of the bone. The bodies of the first and second lumbar vertebræ were found to have been fractured and united by ossific matter. In the second the history of the case was not known, but the fracture was united by ossific matter.

In the museum of the Royal College of Surgeons of Edinburgh there is a fracture of the vertebræ, in the dorsal region, which united by callus, although the patient only survived the accident about two months.

Mr. South has notes of a case in which union of a vertebral fracture, accompanied by perfect palsy of the lower limbs and bladder, ensued, and the patient continued improving for ten years; and he has in his possession two preparations of cases in which the patients lived long enough to have callus thrown out at the seat of fracture. Most practical surgeons will agree with him and Jæger that "many cases of pretended union of fractured vertebræ are very suspicious, and may have been only mere bruising, stretching of the ligaments, or concussion of the spinal marrow." I have heard, indeed, of some wonderful cases of recoveries from fractures of the spine, but one is forced in these cases to doubt the correctness of the diagnosis on reflecting upon how few are the specimens in our museums of cases like that mentioned by Mr. Shaw, where years after the injury pathology revealed that fracture had actually existed.

I would remind my readers also that in some cases of ankylosis of the vertebræ, resulting from disease, the condition is not unlikely to be confounded with ankylosis arising from the effects of injury. Chronic rheumatic arthritis, which has little, if any, tendency to produce ankylosis elsewhere, has a tendency to do so in the spinal column, and that too in a manner which, without careful examination, might be mistaken for ankylosis, the result of fracture. A preparation indeed of this kind, in which ankylosis existed between the second and third cervical vertebræ, was accompanied by appearances so remarkable as to induce some of those who examined it to think that injury, with fracture, had been the cause of it, yet from the history of the case it was certain that it was due to nothing of the sort, but resulted from rheumatic disease, with an unusual

congenital formation. It is therefore possible that some specimens, of the history of which nothing is known, may be supposed to be united fractures or displacements where anchylosis has followed, which really are not so.

It is certain, however, that cases of recovery from fractures of the spine, with symptoms of compression of the cord, are of extreme rarity; one per cent. is greater than Sir Astley Cooper met with in his experience. The proportion of cases in which, after operation, life was preserved shows a per-centage which contrasts very favourably with this.

But it may be said if the correctness of diagnosis is to be doubted in cases which live without operation, is it not equally doubtful in those where patients do not die after the operation. This is of course true, and this is precisely the reason that in attempting to form a fair judgment of the merits of the operation in question I conceive that in one point of view the unsuccessful cases afford stronger testimony in favour of it than those reported as successful. In the one set of cases we remain in the dark as to the precise nature of injury; in the bulk of the unsuccessful cases, where *post mortem* examination left no doubt as to the nature of the injury, enough of improvement has followed to prove incontestably that the theoretic arguments urged against the operation are not well founded. Those cases, however, in which patients have survived the operation have a great value even if it be supposed that in no one of them was a fracture really existing; they show that there is nothing in the operation of trephining the spine which, of necessity, must cause death; they show, if nothing else, at least that the "inevitably fatal" predictions of Sir Charles Bell, and the gloomy forebodings of Mr. Alexander Shaw are equally unverified by facts.

It must not be forgotten that some of the most eminent British surgeons who have ever lived have spoken favourably of the operation of trephining the spine. "Let the young surgeon's mind," says Mr. Shaw, "be fortified against the influence of the authority in favour of the operation given by certain great men in the profession." I would indeed, in the consideration of such a question, repeat let the surgeon's mind be fortified against the influence of authority; let him rather consider the facts of the case, and what is said, than who it is that says it. I refer therefore to the observations of Abernethy, Benjamin Bell, Cline, Sir Astley Cooper, South, &c., not in order to call in the influence of the authority of their names, but to show that surgeons of vast experience regarded



fracture of the spine as an injury of so fatal a character that they judged favourably of an operation which seemed to them to give a chance of saving even one life out of many.

Abernethy, with characteristic caution, says of the operation, "I think that it is a proposition too hardy to be acquiesced in; but I see in it nothing but what is rational—it is the only mode that occurs to one's mind."

Benjamin Bell speaks more strongly:—"Wherever," he says, "the spinal marrow appears to be compressed, and where there is reason to think that the compression is produced by the depression of a portion of bone, as we know from experience that every such case will terminate fatally, if the cause of compression be not removed, it would surely be better to endeavour to raise it, than leave the patient to certain misery and death. By laying the injured parts freely open we may be enabled to raise that portion of bone by which the compression is produced; while, in such circumstances, it cannot add to the hazard of the patient, even allowing the attempt to prove abortive. In a case where symptoms of paralysis were induced by a musket-bullet lodged in the substance of one of the vertebræ, a complete recovery was obtained by extracting the bullet. A portion of depressed bone might often be removed with equal ease and safety; and there is reason to suppose that similar effects would often result from it."

Sir Astley Cooper, as is well known, thought well of the operation. In his lectures he says, that "the proposal is laudable, and the operation not severe, nor does it increase the danger of the patient; time and experience can alone determine its value. If we could save one life in a hundred by it we should deserve well of mankind, and if any good does ultimately result from it, Mr. Henry Cline<sup>a</sup>

<sup>a</sup> In this Sir Astley Cooper was in error; the operation was performed by Louis before Henry Cline's time; and it was more than a hundred years ago, at least, recommended, if not performed, by Heister. He says:—"Wenn aber die Fractur so gross und heftig, dass dadurch das Rückenmark selbst gequetschet und verletzt ist, so folget gemeinlich bald der Tod. Dennoch, weil auch solche Patienten nicht zu verlassen, soll man wohl untersuchen, wie die Fractur beschaffen; und wenn man etwa spühret, dass Trümmer von den wirbelbeinen gantz abgebrochen, selbige herausnehmen: weswegen man aber oft eine Incision machen muss. Solten Stücke von den gebrochenen Beinen das Rückmark drucken muss man solche entweder mit den Fingern oder mit dienlichen Elevatoriis oder Zangen trachten in die Höhe zu heben, oder wo sie los sind, gantz herauszunehmen. Nachdem muss man suchen die wunde von aller Unreinigkeit wohl zu reinigen."—u.s.w. (Laurentii Heister's Chirurgie, &c. Neue Auflage. 1747, 4, s. 184.)

In Dr. James's Medical Dictionary, published more than a century ago, we find



has the merit of proposing it. *Palmarum qui meruit ferat.* That this was not an expression of opinion formed without due deliberation, and uttered, perhaps, hastily in an oral discourse, is clear from the same statement being repeated in his work on Fractures and Dislocations. When speaking of injuries of the spine, he says, "Mr. Henry Cline was the only person who took a scientific view of this accident. He considers it to be similar to fracture, with depression of the cranium, and to require that the pressure should be removed; and, as the cases had proved so *uniformly fatal*, he thought himself justified in stepping out of the usual course, with the hope of preserving life." . . . . "If the operation saves only a life in one hundred, it is more than I have yet seen accomplished by surgery." But more than this, Sir Astley gave practical proof of his approval of the proposal by operating in a case in which there was supposed to be fracture with displacement; it turned out, however, to be merely a fracture of the spinous process, of a dorsal vertebra at its root, and therefore the arch was not meddled with.

Mr. South, of St. Thomas's Hospital, has advocated this operation, which he himself performed on one occasion, although without success. His excellent observations on the subject, in the notes to his translation of Chelius' Surgery, are well known. As regards the violence supposed by some to be necessary in the performance of the operation, and with reference to the patient's suffering, he says:—"I can only say that in Tyrrell's operation which I witnessed, and in that which I performed, no violence was either required or used; in neither did the patient suffer much, nor was his condition rendered worse, but in Tyrrell's case immediately improved." I may add, that in my patient's case the operation was not attended with much suffering; although during a great part of the time he was not thoroughly under the influence of chloroform, but was speaking to those around; the bystanders were surprised at how little he seemed to suffer.

Mr. South discusses the question of operation with great clearness

almost the same; his words are:—"If the spinal marrow is wounded, death follows inevitably; though as it may seem cruel not to attempt the relief of one under these unhappy circumstances, the surgeon should lay the injured part bare by the knife, and elevate the fragments which press upon the medulla, in a proper manner; or when they are quite loose, extract them. Then let him cleanse the wound thoroughly, and apply balsamic medicines, using the napkin and scapulary. He must continue this till the wound is healed or the patient dies."

and candour; and, although led to form a judgment in its favour, he admits what I agree with him in thinking to be the most reasonable objection made to it, as well as by far the weightiest, which can at present be urged against it; this is simply the difficulty of accurate diagnosis in cases of supposed fracture of the spine. But the argument derived from difficulty of diagnosis is common to a great number of obscure surgical affections, without its being considered sufficient to give rise to an established rule, that on that account operative interference is not to be contemplated. If I understand rightly the views of those who are in favour of the operation, they ask nothing more than this: that it shall be acknowledged that in certain cases this operation is justifiable; that it shall not be taught in our schools, that as a matter of course, in all cases of fractured spine operation is worse than useless, and not to be thought of; but that the proceeding of trephining the spine shall be permitted to take its place among the legitimate operations of surgery, as one which although formidable, and in most cases not likely to prove successful, nevertheless holds out by far a better prospect of saving life than the system of doing nothing.

It is in the highest degree interesting in scientific questions to find persons starting from different points, or at least pursuing different routes, yet arriving at the same conclusion. Such is in itself strong testimony in favour of the justness of the conclusion arrived at. When we find, therefore, such a man as Dr. Brown-Séquard, who views the question now under consideration more as a physiologist than as a surgeon, and when we find that as a physiologist he is led strenuously to advocate the operation of trephining the spine, we have in this fact additional evidence in its favour. For my own part, I consider Dr. Brown-Séquard's reasoning upon this subject to be unanswered and unanswerable. I say unanswerable, because I regard Mr Shaw's criticism as quite unsatisfactory. Dr. Brown-Séquard, indeed, goes too far when he says, that "these various operations ought to be employed in *almost all* cases of fracture of the spine;" but when Mr. Alexander Shaw accuses him of "omitting to inform us how the patient—thus all but completely decollated, the muscles of the neck extensively cut, the vertebræ deprived of their posterior arches, their bodies broken across, and the ligaments between them ruptured—should be able to hold up his head, or prevent its rolling to and fro, so as to crush the cord supposed, before the operation, to be sound," he speaks without reflecting; he forgets that in Jones' case, at least, this

“complete decollation” did not occur, and that in truth, the ligaments connecting the bodies, transverse and articulating processes, are hardly ever so entirely lacerated as to permit of its occurrence. Even when the body of a vertebra is artificially severed, the parts are still so firmly held together that the removal of the posterior arch does not materially weaken the union caused by the ligamentous structures. This I can state from having made the trial on the dead subject.

It is quite obvious, however, that in case of operation in the cervical region, a suitable appliance to keep the head and spinal column immovable would be indispensably necessary. Jones speaks of the necessity for some such apparatus in cases like his, and he thinks that the application of some well constructed appliance might have obviated the fatal issue in that instance.

It is needless for me to recapitulate Dr. Brown-Séquard’s<sup>a</sup> arguments; those interested in the subject will peruse them for themselves. But there is one topic of which he speaks of such vast practical importance that I must be allowed to dwell upon it at some length.

The proposition which it is all important to bear in mind in the present discussion, *that death after fracture of the spine usually is due to pressure or some excitation of the spinal marrow, not to partial or complete section of this organ*, will be readily admitted by those who have had leisure to follow the teachings of modern physiology on this subject. They will know that the disturbance of the functions of sensation and motion, are of little consequence in these cases when compared with the disturbance of the vaso-motor functions of the cord. The presiding influence which the spinal cord exercises over the blood vessels, the disturbance of which produces such remarkable effects upon nutrition, secretion, and temperature, is the function, the consideration of which in cases of fractured spine, demands our real attention. Patients do not die because they have lost sensation, or motion in their lower limbs, or half their body. They usually die from the alterations of secretion and nutrition, the principal evidence of which we find in the rapid atrophy, the sloughing and bed sores, the changed condition of the urinary secretion, the inflammation of bladder, ureters, and kidneys, which constitute so terrible a train of symptoms. The experimental physiologist very well knows that morbid excitation of the cord is

<sup>a</sup> Lectures on the Physiology and Pathology of The Central Nervous System. Appendix, p. 245.



the cause of these disturbances, and that a more or less complete section of the cord does not give rise to them, or at least does not cause them to so great a degree, as the prolonged morbid excitation resulting from pressure, or from the driving in of a spicula of bone.

It is therefore a physiological truth which the surgeon should ever have present to his mind, that morbid excitation of the cord resulting from pressure, is the cause of the train of symptoms, which in the cases under consideration, he has the greatest reason to dread. If he can remove the pressure he does all that human skill can do to get rid of the excitation which (unless the injury be high up), step by step gives rise to excoriations, bed sores, sloughing, alkaline urine, inflammation of the bladder, urinary passages, and kidneys. As I have already said, by removing pressure, the surgeon does all that can be done to break the chain of sequences, which when uninterrupted, leads the patient's suffering from fractured spine so inevitably to the grave. But it may be said, the experimental physiologist may have no difficulty in admitting the foregoing proposition; he may know that Henry Cline, and many others, have found that animals recover after section of the cord; he may know that it is not section of the cord but persistent irritation of it from some cause which gives rise to disturbance of the nutritive and secretive functions; but what proof have we that such is the case in man. There is indeed proof enough to satisfy the most sceptical. I have myself seen too cases of wound of the spinal cord in man, made by cutting instruments—one in the London Hospital, another at the National Hospital for the Paralyzed and the Epileptic, London—in neither, although paralysis of motion and sensation existed corresponding to the amount of injury done to the medulla, was there sloughing, alkaline urine, inflammation of the bladder, &c. But in proof of the foregoing proposition I shall content myself by referring to two cases:—

“CASE III.<sup>a</sup>—A drummer of the National Guard of Paris had a quarrel with one of his comrades, who was drunk, and who, not being able to reach him, flung his sabre at him from a considerable distance, just as he was retreating, and had his back towards his antagonist. The point of the instrument reached the upper and back part of the neck. The wounded man at once found his legs give way under him and fell. He was conveyed next day to the

<sup>a</sup> Boyer, “*Traité des Maladies Chirurgicales*,” Tom. vii. p. 9.



hospital of La Charité. The wound, the borders of which were a little contused, was about two inches in extent; it was situated immediately below the occiput, in the upper and back part of the neck and on the right side. The finger could neither discover the depth of it nor penetrate to the vertebral column. The right arm had lost its power of movement, but preserved its sensibility. The right leg seemed somewhat weakened, but was normally sensitive. There was slight difficulty in breathing; pulse frequent, strong and full. He was bled, &c. On the fourth day the weakness of the right leg had entirely disappeared; the patient was able to make some slight motion of extension of the forearm, but could not spontaneously make an attempt at restoring it to the state of flexion.

“On the thirteenth day the patient had recovered his strength and his appetite—he got up and walked; but the paralysis of the upper extremity was the same. When playing tricks with a wardsman who pinched him, he perceived that the left side of the body was insensible. He told me of it the next day, and I observed the following phenomena:—The left lower limb and the left side of the trunk were of the ordinary size, and possessed their power of movement and their usual agility; but the skin of all these parts might be pinched, pricked, and even cut without the patient feeling it or giving the least sign of pain; pins were stuck in to the depth of three or four lines, and when his head was turned away the man did not perceive it. However, extensive contact, as the application of the hand laid on flat and moved over the surface, gave rise to a sort of sensation, but very slight and obscure. This insensibility existed in the entire extent of the left foot, leg, and thigh; it was equally complete over the left side of the abdomen, but it ceased abruptly in front and behind, at the middle line, with this remarkable peculiarity, that in this locality, if the patient was pinched on the left side, he asserted that he felt a sensation, though feebly, at a corresponding point on the right side. A similar demarcation between the right and left side extended to the skin of the penis and scrotum. The insensibility was complete up to the left side of the base of the thorax; but at a little higher level an obscure sensation began to be perceived, which became more distinct higher up, so that at the level of the fourth rib the skin had natural sensibility. The left limb was in a perfectly natural condition. Twenty days after his accident this man left the hospital cured of the wound in his neck, and having no pain or uneasiness in this part; but the right arm, forearm, and hand were almost completely

paralysed, and the left side of the body, excepting the upper limb, were in a state of insensibility which has just been described. Some months after he revisited the hospital; his condition was little changed. The situation of the wound, and the symptoms with which it was accompanied, induced one to believe that the spinal marrow had been engaged.

The second case was one of the most dreadful that I have ever had the opportunity of witnessing:—

“John Neal,<sup>a</sup> a tinplate-worker, was admitted into the Whitworth Hospital, January 31, 1861. He was a thin, delicate-looking man, and presented palpable signs of having suffered severely from syphilis; his skin bore the marks of phagedenic ulceration; the right eye had been the seat of severe inflammation, the pupil was contracted and irregular, and vision impaired; and the left testis enlarged and indurated.

“On admission, he complained of a sense of constriction round his waist, of a difficulty in standing upright, and of general debility. A week afterwards he began to experience difficulty in passing water, and the urine became alkaline and offensive. On the 11th of February he had a rigor, and walked with a staggering gait; and on the 15th, paraplegia was almost completely established, and the sensibility of the lower extremities was somewhat impaired.

“On the 20th, constitutional symptoms of an alarming character were observed, resembling those which follow the infliction of a severe injury. There was cold perspiration over the surface, the pulse was weak and intermittent, and there was frequent vomiting.

“From the 1st of March to the 15th, whilst terrible bed sores were forming, a change was observed in the phenomena of the paraplegia. Some degree of voluntary power was regained in the left foot and leg; the former could be flexed and extended, and the latter drawn up in bed to a certain extent; no such improvement, however, took place in the other limb; but in this the reflex muscular movements regained much of their normal activity, whilst there was a partial recovery of sensibility in both limbs. Whilst, therefore, sensibility was equally impaired in both lower limbs, these extremities were contrasted in the degree of voluntary and of reflected muscular motion which they possessed.

“On the 15th of March, the sloughing had reached a shocking

<sup>a</sup> Case by Dr. M'Dowel, Proceedings of Pathological Society, April 6th, 1861, p. 173.

extent; branches of the gluteal artery poured out florid blood at each dressing, and a large portion of the left sciatic nerve was completely exposed. The sufferings of the patient were now very severe; yet in spite of the profuse discharge, the bleedings and consequent hectic, he lingered until the 2nd of April, when he died, emaciated to the last degree. The ravages made by the sloughs were such, that not only was the sacrum exposed and partly softened, but the small rotator muscles and the gluteal nerve on one side, and on the other even the capsular ligament of the hip-joint, were plainly exposed to view.

“Sixteen hours after death the spinal cord was carefully removed. In the centre of its dorsal portion, but extending considerably to the right of the middle line, a tumour of firm consistence and yellow colour was exposed, when a vertical section of the cord was made. It was of the size of a large pea, of a globular form, and smooth on its surface and section; the cord above and below it was softened, and in parts abnormally vascular. In the centre of one area of vascularity a small yellowish spot was observed. There was no trace of tubercles either in the brain, lungs, or liver. The bladder was contracted and inflamed.”

In one of those cases we see that a partial section of the spinal cord did not occasion to any formidable extent, disturbance of the functions of secretion and nutrition; while in the second, a small tumour producing morbid excitation of the medulla, just as the pressure of bone does, was followed by the most terrible ravages.

In the latter case, had it been possible to know the true state of things, it would have rendered the patient's condition less dreadful to have cut the cord clean across above the disease; and, I believe that in many cases of fractured spine, it would be better for the patient to have the medulla cut across above the injury than to have it left subject to the pressure and irritation of the displaced bone. Let me not be understood to propose such a proceeding; I only say, that even that would be better than doing nothing to remove the pressure, which is the first origin and main spring of what finally kills the sufferer in the great majority of cases.

We learn from Mr. South, that before undertaking the operation of cutting away that part of the arch of the vertebra which presses the cord, Henry Cline proposed to himself four questions. It is interesting to compare the answers given by Cline to these questions, with those which, profiting by the experience



and knowledge gained since then, may now be given to them. Cline says:—1st. “Will the patient die of the operation?—Probably he will, if the injury be severe.” This question, I presume, means will the operation of itself prove immediately fatal to the patient. Cline probably shared the belief, still indeed held by some, that the exposure of the cord or its membranes, or by chance, the letting out of the cerebro-spinal fluid, was excessively dangerous, and likely to cause convulsions and sudden death; he may also, as South suggests, have supposed that exposure of the cord would hasten inflammation and death. We now know that none of the recorded cases have died suddenly; that in no case can it be said that the operation materially hastened the issue to be expected if it had not been performed, and that it has not been followed by meningitis or myelitis, so as to hasten death. In short, no alarming hemorrhage takes place, and in the operation itself, if performed with care, there is no danger.

2nd. “If the cord be much hurt will it recover its functions?—This is unknown; but we do know that if a nerve be divided, it will unite, and the greater part of the spinal cord may be divided in a brute, and yet the animal recover, in proof of which is detailed an experiment which was performed on a bitch. A cut was made at the back of the neck through the muscles, and the yellow substance between the last cervical and first dorsal vertebra having been divided, the handle of a scalpel was pressed firmly down on the cord, and the result was, that the parts behind the division were completely palsied, excepting that the tail continued still capable of a very slight motion, proving that a small portion of the cord was still undivided. With very great care and attention the animal recovered after some months, and resumed all her paces as usual, with the exception of a peculiar movement of her hind limbs in galloping. She was afterwards destroyed, and the spinal cord being examined, was found to have united. This then seemed to be sufficient proof that a spinal cord which had been considerably injured, if not all but entirely torn through, might be re-united and recover its functions.” It is quite certain that the spinal cord may be injured, and afterwards recover its functions. Experiments like Cline's have proved this over and over again. In repeating Dr. Brown-Séguard's well-known experiments on the spinal cord, I have often, more or less, completely cut the cord, and have often seen the animals operated upon almost entirely recover from the paralysis resulting from the incision, and rarely known them to die from a section of a lateral half of the cord.



3rd.—“ After the removal of the arch of the vertebra, will the spine be sufficiently strong to support the body?—Probably it will for ordinary purposes, though the patient may not be able to lift heavy weights.” Louis’s case recovered so as to be able to walk with a stick; none of the other cases regained so much power. The result as regards the power of walking is not mentioned in the short notice of Edwards’ case, given in the *British and Foreign Medico-Chirurgical Review*, but through the kindness of my former pupil, Mr. Phillips, of Cyner, in South Wales, I have learned the following particulars regarding this case:—The patient, whose name was Griffiths, was operated upon a considerable time after receiving the injury; he lived for fifteen years after the operation, but never was able to walk; he regained complete control over the bowels and bladder, and became the father of children.

4th.—“ Will a patient recover from a compound fracture of the spine, which by the performance of the operation it becomes?—The nearer a fracture is to the source of the circulation, and the less compact, and consequently more vascular the bone is, the greater is the probability of recovery. Both these advantages are present in the spine, and therefore favour the successful issue of the case.” Experience has answered this question in the affirmative.

There are of course many questions which arise in connexion with this subject, as regards, for instance, the symptoms indicating cases favourable for the operation, or the reverse, and methods of performing the operation in the different regions of the spine, &c.; but such questions are quite secondary to the great one, as to whether the operation of trephining the spine is justifiable or not. It is to the discussion of this that I have confined myself in this communication, and I cannot help believing that the case I have had to report, although unsuccessful, goes a long way in support of the view which I adopt. I sincerely hope that the perusal of its details will induce surgeons to reconsider a subject which must be admitted to be one of the most important within the domain of surgery.

I have made mention in the foregoing pages of a case in which my friend, Dr. Gordon, has performed the operation of trephining the spine. The full details of the case will be, no doubt, laid before the profession as soon as time makes clear the result. As the case is one, in my mind, of extreme importance, and as the patient’s progress for so far is very gratifying to me, inasmuch as Dr. Gordon was, in a great measure, led to undertake the operation

from a careful consideration of the details of Collins' case, and inspection of the injured parts; I have obtained his permission to state the following:—

The operation was performed on June 3rd, two months after the accident, and in the same region as in my case. I now write on July 22nd. Just seven weeks, therefore, have elapsed since the operation. Of the wound not so much as half-an-inch in length remains unhealed; a minute portion of bone (weighing nearly two grains) came away; no abscess formed; in fact Mr. Shaw's appalling predictions have not been realized. Motion is in an appreciable degree restored in the lower limbs; sensation has improved; a bed sore which had formed on the back, not larger than half-a-crown, is well; the urine which before the operation was alkaline, containing blood, and copious muco-purulent deposit, and very offensive, is now almost natural in colour, and has been for the last month persistently acid; power over the bladder is restored. The patient's appearance is much improved, and his appetite is good.

Mr. Harman, one of the resident clinical clerks in the Whitworth Hospital, has copied from his note book the following, showing the condition of the urine and bladder succeeding the operation:—

Fourth day after operation.—Urine became acid.

Fifth day after operation.—Urine again alkaline, scalds him in passing.

Eleventh day.—Had a motion from the bowels, of the passage of which he was perfectly conscious, but unable to control. Urine since last date alkaline, drawn off with catheter.

Fourteenth day.—Urine when drawn off, acid, and quite free from blood.

Sixteenth day.—Urine slightly alkaline, and continued neutral for three days when it became acid, and has remained so since (now a month).

Twenty-sixth day.—Can eject the urine from the bladder; and since the beginning of July the power over the bladder is such as to force the water strongly against the side of the vessel in a full stream, and on one occasion to a distance of between two or three feet; urine persistently acid.

The catheter is no longer necessary.

Of course I need not say that as regards the life of this patient, the improvement in the urine, bladder, bed sore, and, I may add, the state of the penis, is by far more important than the return of sensation or any return of motor power.

## APPENDIX.

*Reference to the Cases in which Operation has been Performed in order to Remove Bone causing Pressure on the Spinal Cord.*

No	Name of the Surgeon who operated on the case	Reference to where the case is mentioned or detailed
1	Louis, - -	Archives Générales de Médecine, 1836, p. 397
2	Henry Cline, -	New England Journal of Med. and Surg., Vol. IV., No. 1
3	Wickham, (Winchester), - -	Sir A. Cooper's Lectures, edited by F. Tyrrell, Vol. II., p. 20
4	Attenburrow, -	<i>Ibid</i> , p. 20
5	Oldknow, - -	Sir A. Cooper's Treatise on Dislocations and Fractures, new edition, by Bransby B. Cooper, 1842-8, p. 560, Case 352
6	F. Tyrrell, - -	Lectures of Sir A. Cooper, &c., by Tyrrell, Vol. II., p. 11
7	J. Rhea Barton, -	Treatise on Dislocations and Fractures of the Joints, American edition, by J. D. Godman, p. 461; and Malgaigne Fractures, translated by Packard, p. 343
8	Tyrrell, - -	Lancet, Vol. XI., 1827, p. 625
9	Alban G. Smith,	North American Medical and Surgical Journal, Vol. VIII., 1829, p. 94
10	Holscher, - -	Hannoversche Annalen, f., d. ges., Heilk. Bd. 4, 1839, p. 330
11	South, - -	Chelius' System of Surgery, translated by South, Vol. I., p. 540
12	D. L. Rogers, -	Amer. Jour. of the Med. Sc., Vol. XVI., 1835, p. 91
13	Edwards, - -	British and Foreign Medical Review, Vol. VI., 1838, p. 162
14	S. Laugier, -	Bulletin Chirurgical, T. I., p. 401; and Laugier des Lésions Traumatiques de la Moelle Epinière Thèse de Concours, 1848, p. 133, Obs. 52
15	Potter, - -	New York Journal of Med. and the Collateral Sciences, Vol. IV., 1845, March "

No.	Name of the Surgeon who operated on the case	Reference to where the case is mentioned or detailed
16	J. B. Walker, -	A Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement, 1847-8, p. 31, No. 141
17	A. Mayer, - -	Walther's and Ammon's Journal der Chirurgie, Bd. 38. 1848, s. 178. Abildung, Taf. 1, Fig. 1-5
18	G. C. Blackman,	Blackman's edit. of Velpeau's Surgery, Vol. II., p. 392; and J. C. Hutchison in Amer. Medical Times, 1861, July 13, p. 21
19	G. C. Blackman,	<i>Ibid</i>
20	G. M. Jones, -	Medical Times and Gazette, 1856, Vol II., p. 86
21	J. C. Hutchison,	Transactions of the New York State Medical Society, 1861; and Amer. Medic. Times, 1861, July 13, p. 19
22	Stephen Smith, -	Ch. Phelps in New York Journal of Medicine, Vol. VI., 1859, p. 87
23	Blair, - -	Ballingal's Military Surgery, 4th edit., 1852, p. 298; and Essays of Dr. Munro (Secundus)
24	Potter, - -	American Medical Times, January, 1863, p. 17. Case twice operated upon
25	Potter, - -	American Medical Times, January, 1863
26	Heine, - -	Velpeau's Surgery, 1st American edit.

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ART. VII.—*Two Cases of Penetrating Wound of the Chest.*  
By J. H. WHARTON, Surgeon to the Meath and Cork-street Hospitals.

HAVING, in the course of the last session, brought under the notice of the Pathological Society a brief account of a case of penetrating wound of the chest, and exhibited a specimen which displayed the cause of death, I am induced, through the kindness



of Mr. Hamilton, Surgeon to the Richmond Hospital, who furnished me with the details of the case following, to place it, as well as my own, on record. The fact that it occurred several years ago, when Mr. Hamilton filled the office of Resident Pupil at the Meath Hospital, does not, as I think, militate against its interest, but, perhaps, rather increases it.

“James Quin, aged thirty-five, of a strong habit of body, generally temperate, healthy, was admitted into the Meath Hospital on September 24th, 1832, at 8 o'clock, p.m., in the following state.—He had a wound about an inch in length between the cartilages of the fourth and fifth ribs, on the right side, transverse, and communicating with the cavity of the chest, and round the lips of which there was slight emphysema. During each expiration a strong current of air rushed through the wound. Surface of the body cold, particularly the lower extremities; countenance dejected and pale; pulse scarcely to be felt; respiration short and hurried, thirty-four in the minute; great inclination to doze, though very restless; constantly calling for water from the great thirst; lay as easily at one side as at the other; stated that at 7 o'clock that evening whilst endeavouring to protect a woman from her husband, he was stabbed by him with a case knife (he did not examine the knife afterwards), and that he lost a considerable quantity of blood. The lips of the wound were brought together by adhesive plaster; he was put to bed, given a warm drink, and warmth was applied to the feet. A few hours after admission a stethoscopic examination was made, when it was found that respiration was audible over the whole chest; sound on percussion was dull, only over the angle of the right scapula. 1 o'clock, a.m.—Lies with greater ease on affected side, in which he feels a very sharp pain; surface of the body becoming warm; and the pulse is evidently more full, but still quick. 3 o'clock.—The pain and difficulty of breathing being increased, the pulse though still low was yet somewhat fuller, it was thought advisable to take away some blood, which was accordingly done to the extent of six ounces, with great relief, and he shortly afterwards broke out into a profuse perspiration over his face and chest. 9 o'clock.—On percussion anteriorly the chest is quite clear as far as the sixth rib, below which there is dulness. From the axilla downwards laterally, and also at the postero-inferior portion, over the anterior face of the lung, and all round the wound, respiration audible, and only mixed with an occasional sonoro-sibilant râle. On the side and postero-inferior portion it is wanting. No

bronchial respiration nor broncophony; no emphysematous crackling about the wound to-day as was observed last night. Pulse 130, occasionally intermitting. Ordered five grains of calomel and a quarter of a grain of powdered opium in a pill every third hour, and to be bled to sixteen ounces. He was bled; but after a few ounces were drawn away he felt faint, so that the vein was instantly closed. Pulse 120. 3 o'clock.—Pulse 122, full and bounding; skin hot; tongue white; face anxious, flushed, and with a yellow tinge; great oppression; breathing laborious and abdominal. The posterior half of the chest sounds dull, and respiration inaudible; no cough or expectoration; unable to rise in bed. On consultation it was determined to bleed him and continue the pills, and a few leeches after venesection if necessary. Venesection to near thirty ounces was performed before the pulse became softer. The wound was then tied up, after which he felt faint. 8 o'clock, evening.—Much easier; pulse 120, soft; ordered forty leeches to the side, and to continue the pills.

26th.—Countenance looks well; breathing much easier; passed a good night; has taken six pills; bowels confined; tongue white; pulse 120; no cough; less pain in side; on percussing the right side posteriorly and inferiorly it sounds dull; respiration scarcely audible anteriorly, and inferiorly dull; but respiration more audible, though feeble. Ordered thirty leeches to the side, to continue the pills, and to have an enema at bedtime if necessary.

27th.—Going on well; breathing easier; the mouth is now affected by the mercury; the pulse full, but less frequent, 105; tongue covered thickly with white fur; some cough. On examining the chest anteriorly, which alone was done, there is still dulness of the right side, below right nipple, with very feeble respiration and distressing sibilant râles round the wound, which is now open and discharging a bloody spuma. In consequence of this, fresh adhesive straps with lint were applied; much pain in the abdomen, and frequent purging affected him during the night. Last night forty more leeches were applied, the pills stopped, and a draught ordered, containing castor oil, one ounce, and tincture of the acetate of opium, twenty-five drops. 2 o'clock.—Purging and griping continued all day, and are still present; pulse full; lint wet with blood.

28th.—Still some purging, but slighter; has oppression of the affected side, with a sense of weight, and a feeling sometimes of heat, at others of cold; no pain, except a sting in the wound in

turning in the bed; respiration interrupted; and wheezing; no cough; lies most easily on the back; if he tries to lie on either side he feels for a few minutes pain in the wound; the lint is wet with blood, and some oozing from the sides; the chest, anteriorly, looks fuller than the opposite side; tongue loaded; pulse 105, full and strong; feels weak; dulness extends higher up, particularly along the side; posteriorly, respiration more feeble; the affected side is one inch larger than the other by measurement. 3 o'clock.—On consultation it was deemed advisable to enlarge the wound, with a view of giving exit to supposed fluid in the cavity of the chest; no blood or fluid escaped through the wound, nor was any obtained by the use of a syringe or introduction of a bougie far into the wound, or by position. During this operation the patient experienced inconceivable distress; his countenance became anxious and pale; much palpitation; respiration 44; pulse weak. This distress was much abated by the closure of the wound.

29th.—An hour after the operation felt weak, and complained of pain in the lower and back part of the chest, which he had not done before; suffered greatly from anxiety; passed a bad night; at present complains of pains in the side and tenderness; respiration still laborious, and not so evident at the affected side, which appears fuller; dulness on percussion, and feebleness of respiration, if anything, increased anteriorly; not examined posteriorly; respiration in opposite side puerile; some sibilant râles; pulse 126, soft. Last night at 8 o'clock, thirty leeches and the cupping glasses were applied to the lower part of the affected side. Ordered to take two grains of calomel and half a grain of opium three times before morning, provided no diarrhea supervened; in which case they were to be discontinued, and an enema of starch and opium administered.

30th.—Passed a very restless night, constantly changing his position, and getting no relief in any; felt great pain in lower and posterior part of right side; constantly covered with perspiration; respiration short, hurried, difficult, and loud, forty in a minute; pulse 114, and weak; manifestly losing strength, and greatly dejected. On consultation at half-past eleven o'clock, it was determined to make an opening in the lower and posterior part of the chest, which was accordingly done; and when the cavity was penetrated an inordinate quantity of bloody fluid rushed through the wound. This was as dark as venous blood, and did not coagulate on rest; it was forced out during expiration; the fluid not coming

so readily after a little, the tube of a stomach pump was introduced, and a quantity of fluid drawn off by the aid of an exhausting syringe; the patient experienced a good deal of anxiety and distress during the operation; but, though extremely weak, expressed himself relieved and breathed more freely; respiration feeble over the anterior surface of the thorax, accompanied with a moist crepitus superiorly, and an occasional sibilant râle; at the inferior fifth anteriorly, the respiration feeble, and accompanied with a rather dry crepitus, seemingly distant from the surface; percussion elicited a clearer sound; in the upper part of the axilla respiration was loud; inferiorly a sonorous râle, with a sound of two rough surfaces rubbing together, were heard; corresponding to this region the integuments were particularly tender. Half-past 9 o'clock, p.m.—Patient is asleep, breathing quick and short, without so much difficulty as before mentioned, and, without noise; is lying on affected side; appearance much improved; forehead and face covered with a tolerably warm perspiration. 10 o'clock.—He awoke from sleep; says he is much refreshed and easier. Ordered twenty-five drops of the acetum opii; to have a small proportion of wine and water during the night; no oozing from the wound.

October 1st.—Slept little during the night from a teasing cough; he perspired much; his thirst quenched; there was immense discharge from the posterior incision, so as to wet several double sheets. This discharge seemed to be serum slightly tinged with blood; he feels much easier, and has no pain, but is weak; pulse 120; very feeble respiration. 3 o'clock.—Tongue furred, dry; respiration is heard over the anterior part of chest with clear sound on percussion; posteriorly the large half of chest sounds dull. Ordered at night calomel, hippo, and opium, and an ordinary cough mixture.

2nd.—Spent both yesterday and last night very uneasily.

3rd.—The symptoms he had last night increased; he became very restless; his pulse very quick, small, irregular, and intermitting; surface of the body covered with a cold clammy sweat; face pallid; lips blue; a dark circle round the eyes, which were suffused, and looked prominent; at 12 o'clock commenced raving, and died at 7 o'clock this morning.

*Post mortem examination.*—A handle of a scalpel could be passed through the wound; found to be situated between the fourth and fifth ribs, into the cavity of the right side of the chest. The mammary artery was discovered to have been divided, and a small



coagulum filling up both divided extremities. There was a considerable effusion of lymph round the wound, external to the pleura, and the inferior surface of the lung was covered with lymph. The diaphragm was pierced, and the point of the knife had apparently grazed the liver—a spot having been seen corresponding to the wound ecchymosed and lymph. The lung was not wounded—the knife having passed between the middle and lower lobes. The diaphragmatic pleura was covered with effused lymph, and glued to the lung. There was also considerable interlobular effusion gluing the lobes together. A coagulum was found in the cavity of the pleura, and much serous fluid of a reddish colour. The lungs showed that bronchitis existed, but no parenchymatous inflammation.

The above details are so full, and the symptoms, progress, and termination of the case so similar to that of John Hart, which came under my own observation, that it is only necessary to note and compare their principal features, which I shall most readily effect by means of the following arrangement, and thus save the reader's time.

#### CASE OF JAMES QUINN.

Patient's age, 35; habits *generally* temperate; wound 1 inch in length, between the cartilages of the fourth and fifth ribs, on the right side; transverse in direction; *unmistakably known* to communicate with the chest; emphysema present; on expiration, current of air through wound; temperature of the body cold; countenance dejected, pale; pulse scarcely to be felt; respiration short, hurried; thirst constant; decubitus at either side. Local treatment:—Lips of wound were brought together by adhesive plaster; stethoscopic examination, made anteriorly and posteriorly; respiration audible over the whole chest; sound, on percussion, over the angle of the right scapula, dull; sharp pain in affected side; venesection and extensive leeching practised.

Patient placed under the use of mercury in a few hours after admission to hospital.

#### CASE OF JOHN HART.

Patient's age 22; habits *dissolute*, and habitually intemperate; wound one inch in length, between fifth and sixth ribs, on the left side, the external margin in a line with the nipple, transverse in direction, not *unmistakably known* to communicate with the chest; no emphysema; no current of air through wound on expiration; temperature, appearance of the countenance, state of the pulse, respiration, thirst—similar; decubitus on the back. Local treatment similar—a compress and bandage having been used in addition so as to throw out of action the intercostal muscles; no stethoscopic examination made, except anteriorly, over and above the bandage. In the latter situation, a very clear tympanitic sound was discovered on percussion, as if produced by pneumothorax; respiration, audible over the whole chest anteriorly; sharp pain in cardiac region; no venesection practised, and a single application of a few, six, leeches made use of; no mercury used until the fifth day after admission and receipt of the injury, and then, in consequence of the occurrence of a friction sound over the left

Bleeding from the wound externally on the third day after admission.

On the fourth day evidences of internal hemorrhage were so manifest, particularly from enlargement of the affected side, that the wound in the chest was incised for the purpose of giving exit to the contained fluid. As this operation was not successful an opening was made on the succeeding day into the chest, on the affected side, at its postero-inferior portion, when an inordinate quantity of bloody fluid was removed. Its colour was that of venous, and it had not the property of coagulating; temporary relief ensued; the patient survived the operation till the third day after its performance, and lived nine days after the accident.

anterior portion of the chest, in doses of one grain of calomel and half a grain of opium, at regular intervals; subsequently antispasmodics in cough mixtures were exhibited. Prior to this date Battley's liquor of opium was freely administered, and continued for four days; ice was largely and constantly prescribed, and iced beef-tea and iced brandy and water for diet. No bleeding from the wound externally till the sixth day after admission, as shown by the dressings remaining free from blood stains. No evidence of internal hemorrhage till seventh day after admission, when, in consequence of a violent attack of delirium, similar to delirium tremens, the patient rushed out of bed; respiration, which was up to this time *comparatively* tranquil, became hurried, and the dressings were freely stained with blood; dulness by degrees replaced the clearness which had previously existed along the left axillary region, and anteriorly, rendering it but too evident that that side of the chest was becoming more and more occupied by the presence of a fluid, by means of which the heart was gradually displaced towards the right side. All medical aid having proved abortive, it was determined to perform the operation of paracentesis; forty-eight ounces of fluid, precisely similar to that removed in Quinn's case, were drawn off, but without relief. The patient survived the operation three and a-half hours, and the accident fourteen days.

### *Post mortem Examination.*

#### CASE OF JAMES QUINN.

Direct communication of external wound with the internal; the right mammary artery was divided.

Coagulum found in the divided extremities of the vessel.

Considerable effusion of lymph round the wound external to the pleura, and on the inferior surface of the lung. The lung

#### CASE OF JOHN HART.

Indirect communication; the trajet of the wound passing in a direction downwards and inwards, anterior to the cartilages of the sixth and seventh ribs, which were divided near their junction with the sternum; the mammary vessels of the right side being necessarily severed; no trace of coagulum in the vessels, which were empty and flaccid; no lymph round the wound or elsewhere, except on the postero-inferior surface of the sternum, and this deposit was soft, in small quantity,

was not wounded, but much serous fluid of a reddish colour was found in the cavity of the chest, and also a coagulum.

and presented the appearance of a semi-solid pus. The lung was soft, engorged, and collapsed, and neither it nor the pericardium was wounded; a considerable amount of fluid of the same character as that mentioned above, and a small clot, of a dark colour and soft consistence, was found at the bottom of the fluid.

The difficulties attendant upon the management of penetrating wounds of the chest, with accompanying hemorrhage, are so well known to surgeons, that even were sufficient space at my disposal, it would be unnecessary to refer to them, at least upon the present occasion, I shall, therefore, content myself by alluding, and that with the utmost brevity, to two points, which are, I think, worthy of more consideration than they have generally received.

First.—That sufficient importance is not paid to the necessity of maintaining the blood in such a condition as to favour its coagulating property. If this view be at all correct, it will follow that great caution should be exercised in the use of such means as will be likely to diminish to any appreciable extent its healthy proportion of fibrin. Hence I regret that in the management of this case I had recourse to the exhibition of mercury even to a limited extent. Secondly.—That the same remark holds good with reference to the nervous, circulating, and respiratory systems, the *due* control of which constitutes in itself a not unimportant means of checking hemorrhage, and of controlling the tendency to inflammatory action. Accordingly, in the case of John Hart, opium was so administered as to produce a continuously seminarcotic state for four days, during which period the patient was wonderfully free from any symptoms of an untoward nature. I was induced to have recourse to this drug, and to place my chief reliance upon it, from the marked benefit which followed from its exhibition in a case of accidental rupture of the lung, with pneumothorax and hemoptysis, which terminated favourably; and from a consideration of its well known efficiency in certain lesions of the viscera, where the object of the surgeon is to obtain rest of the organ implicated.

ART. VIII.—*Observations and Cases setting forth the Advantage to be derived from the decided use of Caustics in certain Surgical Diseases, more particularly those affecting the Bones and Joints.*  
By FREDERICK KIRKPATRICK, M.B., Fellow of the Royal College of Surgeons in Ireland, and Surgeon to the Hospital Wards set apart at the North Dublin Union for the Treatment of Surgical Cases.

THE history of modern conservative surgery during the last ten years, and the successful results that have been attained by the bold use of the knife in cases of carious bone, or diseased articulation, are so strikingly calculated to excite the admiration of the surgeon, and to stimulate his ambition to follow in the path of operative surgery, that I am diffident of obtaining attention to the mode of treatment which it is my object to advocate in the following pages, more particularly when no claim is laid to original conception, but merely to the more-extended and freer use of a well known remedy. The hospitals of the North and South Dublin Unions afford an unusually large and varied field for the study of disease, and especially of those under consideration; for inasmuch as the general hospitals of the city prefer to admit acute cases that promise a tolerably rapid cure, and that present features favourable for operation, the majority of the chronic cases of disease, consequently, seek relief at the hospitals of the North and South Dublin Unions, where in general they remain the subjects of lengthened observation; and I have thus, during the last twenty-five years in which I have held the post of surgeon to the North Union, had full opportunity of testing the effects of treatment on the various forms of chronic disease, and I have obtained such rapid and excellent curative results from the use of caustics, particularly of the potassum calce, that I am induced to lay a brief statement upon the subject before the profession, and to detail some cases where the use of caustic, combined with incision, in the treatment of diseased bones and joints, was attended with remarkable success. For many years I have preferred the practice of opening all chronic abscesses by means of caustic, first making an eschar, and then opening the abscess by a small crucial incision through its centre, and in the case of large abscesses making two or three eschars, and an opening through each. I have also found great advantage from its use in the treatment of scrofulous abscesses, and sinuses in the neck, groin,



and axilla, also in fistulous disease, in anthrax, and in many cases of ulcers, particularly those of a phagedenic character. The principle of combining incision with caustic, which was followed out at the Middlesex Hospital, in the treatment of cancerous diseases, by means of chloride of zinc paste, first suggested the idea to me, and I have constantly since had recourse to the practice, both in the use of the chloride of zinc in cases of cancer, and of other morbid growths, and of the potassa cum calce in diseases of the bones. With the aid of incisions I have thus been enabled to deepen the slough from day to day, and extend its action as deeply and fully as necessary. I have in this manner treated several cases of disease of the carpal and tarsal bones, and of the smaller articulations of the hands and feet. I have also derived much advantage from the practice in cases of necrosis of old standing in which I used the caustic freely, converting the small fistulous orifices into large funnel-shaped ones, and again and again, at intervals of a few days, carrying its action deeply down to the diseased bone. In some cases the sequestra have been thus divided and easily extracted in portions through the enlarged openings, in others the caustic has induced such a formation of plastic material that the cases have got well without the visible escape of any particles of magnitude. This treatment recommends itself very strongly in a large class of patients, where, from broken down health, and the co-existence of organic disease of internal organs, operative proceedings are so unpromising.

The efforts made by nature to repair disease, so wondrously perfect in wounds and fractures, and, I may say, in nearly all the more acute cases of surgery, are often most ineffectual and tedious when directed to the relief of diseases of the bone. Frequently do we see a patient worn down by hectic, in consequence of a caries, of probably small extent, in the bones of the sternum or scapula, or in one of the pelvic bones; abscess after abscess has formed, small particles of bone have at intervals been washed out of the fistulous openings by the wasting discharge; but after years of suffering the patient is still unrelieved, and the diseased bone is so surrounded and enclosed by a dense, highly-organised, and vascular structure that any surgical proceedings to reach and remove it with the knife is attended with great difficulty, and considerable loss of blood. In all such cases the treatment I propose offers a prospect of speedy relief. In particular I advocate its use in caries of the carpus and tarsus where excision has not met with the same measure of success

that its use in the larger joints has been attended with; you can attack each diseased bone separately, boldly boring down to it with the solid stick of caustic, and stimulating its diseased structure to take up a healthy action. In this manner two or three openings can be made, extending deeply down into the diseased bones, without inflicting any serious injury either on the blood vessels or the tendons; the latter can usually be seen when the sloughs have come away, and if the openings are made of sufficient size the bone can also be seen, and examined with a view to the removal of any loose portions.

The number of bones entering into the construction of the wrist and ankle joints, and the communication between their synovial sacs, has always created a reluctance on the part of the surgeon to interfere by operation; and where excision has been attempted extension of disease has been more frequently the result than cure, and in too many cases dangerous consequences have ensued from tetanus, pyemia, or the spread of unhealthy inflammation; consequently the generality of surgeons have been satisfied to trust such cases to the treatment by iodine, rest, and constitutional remedies. The length of time that such a case may last, and the very imperfect hand or foot that remains to the sufferer when consolidation is at last attained, are but too well known to all surgeons. The *Lancet*, March 25th, contains an interesting paper by Mr. Joseph Lyster, of the Glasgow Hospital, on this subject. He, after commenting on the unpromising results of partial operations on the carpus, recommends the removal of the entire carpus, together with the articulating extremities of the radius, ulna, and metacarpal bones, taking away fully two and a half inches from the length of the limb. Mr. Lyster gives several cases of this operation which he performed, that will repay perusal. His operations were formidable ones, and the resulting limbs must have been very considerably altered from their original fair proportions. His paper at all events proves the truth of my proposition, that partial excision of the carpus is not looked upon as a promising operation, and gives me confidence in proposing to the profession the treatment by caustic and incision, aided by well-considered methods for attaining perfect rest to the affected limb. I by no means wish to revive the use of the superficial caustic issues over diseased joints, as I believe it to be a source of hectic and additional waste, and a practice that has been, in consequence, properly abandoned; but used in the manner I advocate, for the destruction of diseased and undermined

integuments, for the opening up of sinuses and fistulous communications with diseased bone, and for the breaking up and stimulating of the carious portion of the bone itself, it is a remedial agent of great potency, and one which I can speak of with the fullest confidence. When brought in contact with carious bone it does not produce much pain, but its action on the skin and soft parts is attended with so much suffering that chloroform should always be administered; however, as the application occupies so short a time, and the resulting pain is so soon removed by cold water, it is not necessary to push chloroform to any extent.

In conclusion, I beg to express my expectation that this mode of treatment will be found equally applicable to the diseases of the larger joints, in all cases where the disease commences in their articulating extremities, and I fully purpose making trial of it whenever I meet cases that present the symptoms of inflammatory action going on either in the great trochanter or in the heads of those bones which form the elbow and knee-joints.

#### CASES.

Mr. R. S., aged seventeen years, consulted me in April, 1864, for disease affecting all his extremities. I found him in bed, wasted with hectic fever, and his hands and feet enveloped with poultices and oil silk. There were five sinuses leading down to diseased bone in the carpal and metacarpal articulations of the left hand; the right hand was similarly affected, but in a less degree. In the right foot the metatarsal bone and the first phalanx of the second toe were diseased, together with the metatarsal bone of the fourth toe. In the left foot there was necrosis of the metatarsal bone and phalanx of the great toe, and caries of the metatarsal bone of the second toe. Unhealthy sinuses, with undermined skin surrounding them, led down to all the carious bones.

In this case I freely used the caustic treatment, destroying all undermined skin, and carrying a pointed stick of potassa cum calce down to the bone at each spot where caries existed, and making a free direct opening. I stopped poulticing and dressed the sores instead with cotton and salad oil. I saw this gentleman twice a week, and frequently repeated the use of the potassa cum calce to keep the openings free, stimulating the deeper parts by means of astringent lotions and ointments, and with probes coated with nitrate of silver. This patient recovered so rapidly that he was

out fishing in June; in July he was on horseback; and by the end of September all his sores were well excepting the great toe of the left foot, which had been considerably enlarged by necrosis, and was kept irritated by the pressure of his boot; here a small fistula remained during the Winter, but was reduced to the size of a pin hole by April, 1865—one year from the commencement of the treatment. This patient is now the picture of health, but, as a matter of precaution, I have placed an issue under the left clavicle.

E. B., aged twenty-four, of delicate aspect, and labouring under secondary syphilis, consulted me in April, 1861. He had some time previously received a wrench in the wrist of his right hand, which accident had been followed by pain, swelling, and loss of motion. The hand was now pale and puffy; he was quite unable to use the fingers or bend the wrist, and the slightest touch, more particularly of the fore-finger and thumb, gave him exquisite pain. Having made up my mind that the disease was confined to the carpal extremity of the radius, I proposed to bore down upon it at once with the potassa cum calce, and the patient assenting, I made an eschar the size of a shilling over the painful spot, and in two days a small crucial incision in its centre, through which I introduced a pointed piece of potassa cum calce deeply down to the diseased bone. In three weeks, during which time the hand was kept at rest in a splint, the sloughing process was completed, and a clean circular ulcer about the size of a sixpence remained. The acute pain had almost ceased. I then strapped carefully each finger with straps of linen moistened with an isinglass cement, which I use for this purpose, and confined the hand and wrist in an immovable case of the same material, leaving the upper surface of the wrist open for the dressing of the sore and the use of the caustic when necessary. In this instance the use of the potassa cum calce was repeated but once, and that slightly. The ulcer healed up firmly, all pain ceased, and in three months the bone disease was evidently cured, the patient only having to contend with the stiffness of the fingers and the pain inseparable from the attempt to restore their motion. From this his recovery was slow, so that a year elapsed before he was able to completely close his hand, but he ultimately recovered its perfect use.

In the month of September, 1864, H. H., aged fifteen, after over-fatigue, was attacked with pain in his right foot, and became unable to walk. He was treated, for two months, in the country,



and was told that his case was only a bad sprain. I saw him on the 1st of November. He was then quite unable to bear on the foot, which was pale, œdematous, and had altogether lost its healthy aspect. The disease was confined to the metatarsal bone of the great toe, and there was evidence of the formation of matter under the fascia. Having decided on using the caustic, I made a large eschar over the suspected spot on the dorsum of the foot, and incising its centre, I at once carried the caustic deeply down to the diseased bone; an escape of thin matter took place, relief from pain followed almost immediately. The separation of the slough occurred in fifteen days, leaving a large free circular opening down to the diseased bone. November 20th.—There was a puffiness observed on the inner surface of the foot, I therefore made a second free opening with the caustic down to the diseased bone, and afterwards at intervals of ten days or a fortnight, I used the caustic slightly to obviate the great tendency in the orifices to heal; at the same time the openings were dressed deeply with stimulating ointments. The discharge gradually ceased, the ulcers healed from the bottom in the most satisfactory way; and early in May the patient returned to the country, able to bear on the foot without pain, and having every prospect of a perfect recovery.

Anne Dowd, aged 25, admitted into Hospital January 12th, 1865. Her right leg was greatly swollen from the knee down; over the inner ankle there was a large gangrenous swelling, very like an anthrax; and along the course of the tibia were several fluctuating tumours in a state of high inflammation; over the external malleolus there was also an abscess, and the apprehension was entertained that the disease communicated with the ankle joint. The least motion of the foot produced agony. The imperfect history obtained from the patient was to the effect that twelve years back she had been confined to bed for two years with necrosis of the affected limb, but that she had made a good recovery, and remained well until a fortnight before admission, when in carrying a heavy load she received a wrench that was followed by inflammation and violent pain above the inner ankle, where an abscess followed, and the inflammation extended up the leg. This case was so unpromising at first, that the question of amputation was canvassed, and for several days I treated it as I would a compound fracture, keeping the limb well supported, and raised above the level of the body. When the great swelling had in some measure subsided, there remained considerable enlargement of the tibia, and

along its surface were four collections of matter, in addition to the large sloughy opening that seemed to communicate with the ankle joint, and into which a finger could pass for fully the depth of an inch.

On the 5th of February I put this patient under chloroform, and used the caustic freely over the thinned and diseased skin, making five large eschars, which on their separation left free openings down to the bone.

February 23rd.—Marked subsidence of pain, inflammation, and swelling. The two upper openings nearly closed from the bottom. The three lower openings not healing up in such a satisfactory manner, I therefore put the patient under chloroform, and used the caustic again, destroying all diseased and undermined integuments, and rotating the solid stick of potassa cum calce as deeply as I could into the diseased openings.

March 20th.—A small piece of bone came away from lower opening.

25th.—Patient can move the ankle joint slightly in the direction of the limb, but no lateral motion is permitted. All the openings healing up from the bottom in a healthy manner. Patient getting about the ward on crutches.

Left the Hospital on the 14th of April, walking with the help of a crutch. All the openings firmly closed up.

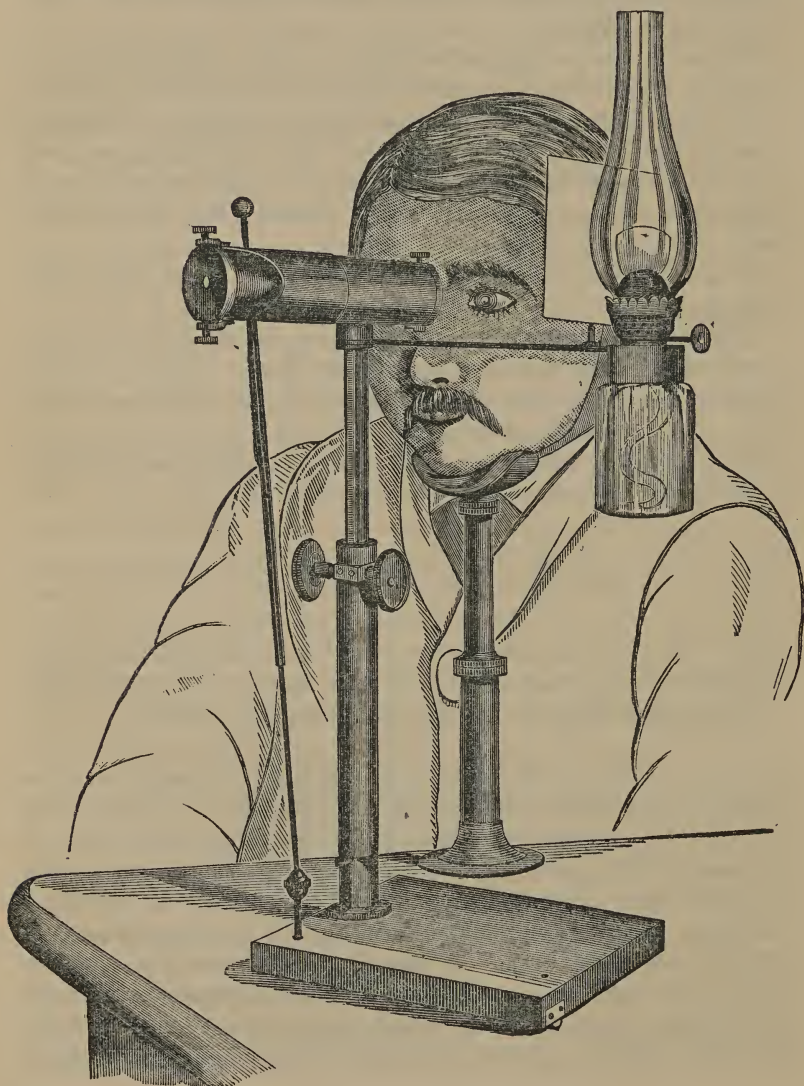
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ART. IX.—*On Smith, Beck, and Beck's Modification of Dr. Liebreich's Ophthalmoscope.* By E. PERCEVAL WRIGHT, M.D.;  
Ophthalmic Surgeon, Dr. Steevens' Hospital, Dublin.

DURING the Winter of 1864, when engaged in lecturing on some of the diseases of the eye, I found it impossible to demonstrate to several of my pupils certain morbid changes in the retina, on account of their want of practical acquaintance with the use of the ordinary hand ophthalmoscope. In this emergency I thought it advisable to employ an ophthalmoscope which would enable me to show, with some degree of certainty, not only the ordinary appearances of the retina, but also the same appearances to several students in succession. For this purpose I wrote to my friend Mr. R. Beck, of the firm of Messrs. Smith, Beck, and Beck, of London,

suggesting to him to make for me a Liebreich's ophthalmoscope, or to modify it so as to make it more portable. This he most kindly undertook to do, and he forwarded to me, in a very short time, the instrument which I proceed briefly to describe; one which I think, when known, will be very extensively used in this country.

Liebreich's ophthalmoscope consists of two short tubes, moving





one over the other by rack and pinion. The tube nearest to the observer has a piece cut out of its side at the free end, where hangs a small metallic concave mirror on trunnions, in such a manner that it turns readily on its vertical axes, and can be removed with facility. The other tube, turned towards the patient, carries at its free end a convex lens of about two inches focal length, hung in the same way as the mirror. This latter tube is securely united to the upright stem. The optical arrangement is the same in the instrument figured as in Liebreich's, but the mechanical construction is simpler; it can be used on a table without fixing; the adjustments are easily and quickly made; and owing to the lamp being attached to the stem of the ophthalmoscope by an arm, the illumination remains unaltered however much the instrument may be moved about during the necessary adjustments.

The chin rest is detached and will stand of itself, the height being regulated by a sliding tube, which may be fixed in any position by the milled ring. The ophthalmoscope is mounted upon a mahogany board, which has three small rollers underneath, so that the whole of the instrument may be moved in a direct line, nearer or further from the eye, at the same time allowing of any side movement that may be required, or of a slight rotation of the instrument, which is sometimes necessary when light is reflected from the surface of the cornea. The height of the horizontal tube carrying the mirror and the lens, is regulated by the milled heads which are connected with a rack and pinion. The mirror, when in use, is drawn out about an inch from the upright stem; and the lens is so rotated as to throw aside the reflexions from the surfaces of the glass. The lamp is attached to the stem by means of an arm; a shade fits into a hole near the lamp, and prevents the direct light from shining on the patient's eye. The object upon which the eye to be examined should be fixed, consists of a small knob of ivory, which can be moved in any direction by means of its sliding tube and the ball and socket joint at its base. This ophthalmoscope packs into a very small case, and can be carried about with the greatest ease. The best oil to burn in the lamp is some of the best refined paraffin oil.

In all cases when it may be deemed expedient to use a fixed ophthalmoscope, I have no hesitation in asserting that this improved instrument of Messrs. Smith, Beck, and Beck will be found one of the very best. The workmanship and finish leave nothing to be desired.



## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*The Personal Responsibility of the Insane.* By JAMES F. DUNCAN, M.D., T.C.D., &c. Dublin: Fannin and Co. 1865.

DR. DUNCAN has been long and favourably known to the profession in connexion with the medical treatment of insanity; and his special experience has been such as to entitle any literary production of his on this subject to more than ordinary attention. The work before us, published in Messrs. Fannin's usually good style, comprises 98 pp., small 8vo; and we feel bound to say that a perusal of them will greatly interest the scientific reader, and this quite as much as the general public, for whose information this essay is more particularly intended.

In the opening part of the essay Dr. Duncan discusses the difference between insanity and crime, and observes:—"What, then, is the difference between insanity and crime? It is simply this—insanity is a morbid condition of some part of the material organization connected with the mind, while crime is a perverted action of the mind itself. The one is properly a disease, and belongs to the province of the physician; the other is a change independent of disease, and belongs to the province of the moralist." The essence of insanity in every form, shade, and variety, is defined to be "some deviation or other from the healthy condition of the corporeal organization which disturbs the free action of the mind in some of its faculties, and renders it incapable of discharging its functions in a proper manner."

We shall give his own description of what is, perhaps, the most prominent idea in Dr. Duncan's essay. In reply to the question—Where is the line to be drawn which divides the region of responsibility from that of irresponsibility? he observes:—"Two very opposite opinions prevail upon this point—one is that every form and degree of insanity necessarily excludes the idea of moral accountability in the offender; the other that nothing but actual frenzy, or the total loss of intelligent consciousness, produces this

immunity. The first opinion is commonly entertained by the masses of society, including generally the members of the medical profession; the second is more commonly held by legal writers and by judges on the bench."

He proceeds to show that in practice the former opinion is generally acted on in our courts of justice; while on the other hand, the decisions on cases similarly circumstanced are various and conflicting. Further, we find an elaborate discussion on the divisibility of the mind into faculties; and on this matter Dr. Duncan makes no *ex parte* statement, but carefully weighs the plausible arguments usually advanced on both sides of the question. Respecting this part of the subject he observes:—"The settlement of such questions as these is of the greatest importance in reference to the subject discussed in these pages. If the opinion be correct, that the mind is indivisible, and that it must act as a whole whenever it acts at all, it will necessarily follow, as Lord Brougham has asserted, that there can be no such thing as partial insanity; the mind, if unsound in any respect, must be considered unsound in all its operations, even when acting to all appearance in conformity with the requirements of sound mind." Again, he observes:—"The generally received opinion on this subject, in the present day, notwithstanding the dissent expressed by the learned authorities to whom I have alluded, is that the mind, like the body, consists of a variety of distinct faculties, corresponding in number to the different functions they have to perform. A careful study of its operations in health and disease, as well as analogy drawn from the structure of the body, leads us to this conclusion; yet for the reasons stated it can be looked upon only as a matter of inference and not as a truth established upon evidence, not to be set aside." Dr. Duncan proceeds to explain how it is, that in cases of alleged partial insanity medical men so frequently give conflicting testimony. This he shows to proceed from the simple cause that one man who examines the case is put in possession of the clue to the delusion under which the patient labours; *he* immediately arrives at one conclusion. A second medical man, not having been put in possession of the precise point upon which the partial derangement turns, after a long and close conversation with the patient, comes away with the impression that the suspicion of insanity is unfounded.

We have neither time or space to follow Dr. Duncan through that large part of his volume which treats of the legal test of

insanity—"Knowing right from wrong." Suffice it to say that he shows the said test to be difficult of application in most cases, while it is so vague as to be practically useless in determining the point for which it is relied on.

He also ably refutes the dictum of Lord Westbury, that insanity is a fact which can be ascertained by evidence like any other fact.

We now come to review the latter portion of the essay, which is divided into sections, the first of which is headed—"Of insanity Characterized by Delusions." In reply to the question—What is a delusion? Dr. Duncan observes:—"There are three distinct features that every delusion ought to possess in order to its being considered evidence of an unsound mind. It ought, in the first place, to exhibit a certain degree of permanence. . . . In the second place, it must be proof against conviction, so that arguments which are sufficient to satisfy ordinary minds of its erroneousness, fail to change the views of the person who holds it. And lastly, it must influence the conduct of the individual. Without this latter characteristic, however absurd in itself, its harmlessness will prevent its being regarded as an evidence of mental derangement."

Under the next sectional heading—"Of Moral Insanity," Dr. Duncan very truly remarks:—"The name given to this form of the complaint is singularly infelicitous." Doubtless it is, because, as afterwards remarked, it suggests the idea of vice and depravity as an essential feature of the species; whereas many persons affected by it are blameless in their conduct, and exhibit sound judgment upon all questions of morality. What is to be understood by the term is "a perversion of the moral feelings, including the affections, passions and instincts, as apart from and unconnected with any lesion of the intellectual faculties."

Distinct from that habitual want of cheerfulness which distinguishes many of the subjects of chronic lunacy, Dr. Duncan gives a section of his essay to "Insanity Characterized by Depression of Spirits." This he considers to be frequently an acute affection, temporary in its duration, and recurring at intervals in the same individual. There is also a lengthened section devoted to the discussion of "Impulsive Insanity," as well as one on "Insanity Characterized by Various Morbid Propensities." These, Dr. Duncan properly observes, should be included under the general head of moral insanity, to which such complaints as pyromania and dipsomania are generally considered to belong. We shall not here pause to discuss the important social bearings of dipsomania

in connexion with moral insanity; as in a former number of this Journal the entire subject has been treated in a review entitled "Reformatories for Drunkards;"<sup>a</sup> we may, however, state that Dr. Duncan treats them with that ability and moderation which we look for in vain among the riders of hobbies, especially among such as are monomaniacs on the subject of total abstinence from spirituous drinks. The last section, headed—"Of Coroner's Inquests," is a most sensible protest against the indiscriminate verdict of "temporary insanity," which mistaken charity almost invariably pronounces in cases of suicide. Dr. Duncan clearly shows that the knowledge that such verdict will almost certainly follow a suicide, is most likely to induce an otherwise hesitating man to commit this fearful crime. We shall give whole and entire Dr. Duncan's *conclusions* regarding the subject of his essay:—

"I. Persons labouring under ordinary mania, where there is incoherence, excitement, and the general symptoms of complete mental estrangement, are thereby rendered entirely irresponsible for whatever criminal actions they may commit in that state.

"II. Persons labouring under the various forms of partial insanity are irresponsible for the criminal actions they may commit, which are plainly traceable to that condition, even though such actions may not admit of justification, on the hypothesis of the causes which provoked them being real instead of imaginary.

"III. When criminal acts are committed by persons partially insane, although no connexion may be traceable between these actions and their insanity, their condition may be looked upon as a ground of mitigation of punishment, and treated accordingly.

"IV. When persons convicted of crimes are exculpated, either wholly or partially, on the ground of insanity, their condition requires them to be transmitted to a proper hospital or asylum for their immediate medical treatment.

"V. That, subject to proper precautions, as soon as such patients are restored to health, reason and humanity require that they should be set at liberty, unless there should appear to be special risk of a relapse.

"VI. That when unoffending persons have suffered personal injury from the violence of insane persons, reasonable compensation should be made to them out of the lunatic's estate.

"VII. That Coroner's Juries should not be expected, or required to

<sup>a</sup> See the Number for May, 1862. As its author has since then had it reprinted as a pamphlet, with his name affixed, we may add, that it was written by Dr. Belcher, a frequent contributor to this Journal.



append to their finding in cases of suicide, the usual formula, 'when labouring under temporary insanity,' unless some urgent necessity arise to require the person's state of mind to be carefully investigated, and the fact of such condition existing has been clearly established.

"VIII. That unless in clear cases of positive mental derangement some modified form of the obsolete mode of un-Christian sepulchre should be maintained as a means of checking the too prevalent tendency to suicide."

Shall we criticise the style of this essay? In our opinion it is remarkably well written, and in no instance does the writer fail to convey his meaning in concise, plain, and intelligible words.

We have noted but two slight verbal inaccuracies. On p. 61, writing of *three* characteristics of delusion, he speaks of the *third* as "this *latter* characteristic," meaning, of course, the *last* of three. Again, in the section on coroner's inquests and in conclusion VIII., as above given, the word "sepulchre" is curiously used; and is doubtless intended for "sepulture."

These trifling words, however, tend to show how well written the essay is, as the exception proves the rule. We commend it heartily to the profession and to the educated public. Its style, composition, and mode of arrangement is such as we should expect from a scarlet gownsman of our National University; and its matter is in keeping with the motto of our Irish College of Physicians, of which Dr. Duncan is a Fellow, namely—"Ratione et Experientiâ."

*The Works of Sir Benjamin Brodie, Bart., D.C.L., Serjeant Surgeon to the Queen, President of the Royal Society, &c., with an Autobiography.* Collected and arranged by CHAS. HAWKINS, F.R.C.S. Eng. In 3 Vols. London: Longmans. 1865. 8vo, pp. 659, 680, 697.

IN the Introduction to this edition of the works of the late Sir Benjamin Brodie the editor tells us that he considers the publication of a collected edition of his works the fittest memorial of his friend and teacher. "No public memorial has yet," he says, "been raised to this great surgeon. When the Wellington Despatches appeared a distinguished foreigner remarked, 'What a noble monument Colonel Gurwood has raised to the Duke—one more enduring than

marble or brass.' The task of arranging such a monument to the best of friends and most indulgent of instructors has been to me a labour of love." We cordially agree with Mr. Hawkins that such a monument is the most worthy of the fame of Sir Benjamin Brodie, and the best calculated to stimulate others to adopt the course that led him to the highest pinnacle of social and professional reputation, gained for him the admiration and esteem of all his contemporaries, and made his name one that must be known so long as human flesh has ills to bear, and wherever the healing art is cultivated or the sciences on which it rests are studied.

The works collected by Mr. Hawkins embrace Sir Benjamin's earliest as well as his latest writings, and all have their value; for, though some of the earlier physiological essays may fall short of the present state of science, they were very important advances on that of the period when first published, and the facts established served as firm starting-points whence further researches took their start. The works of Sir Benjamin Brodie are, however, too well known to require any notice here; we shall rather devote the space at our command to a brief sketch of the very interesting autobiography with which they are introduced.

Many of the successful men whose lives have been written have been men who have risen to eminence in spite of the most adverse circumstances; but the history of Sir Benjamin Brodie is that of a man surrounded by the most favourable circumstances; his great merit consisted in his having cultivated all the advantages he was possessed of. Instead of letting these lessen his own individual efforts, he regarded them as stimulants to exertion, and used them as stepping-stones to further progress. Sprung from a father whose great talents and acquirements seem only to have been surpassed by his judgment, he received his early education at home; and, while instruction was not neglected, education in the true sense of the word was the great object, for, in addition to a good knowledge of classics and the elements of mathematics, chemistry, and other sciences, he was led to form habits of application and industry, of reflection and self-reliance.

Prepared by such a training, and firmly impressed with the knowledge that his livelihood depended on his own exertions, young Brodie proceeded to London in 1801, being then in his eighteenth year, to enter on the study of surgery, in obedience to his father's wishes, who was led to guide his mind to that profession by the great success of Dr. Denman, who was married to his sister, and of

Dr. Baillie and Sir Richard Croft, who married his nieces. In alluding to this important era in his life Sir Benjamin takes occasion to express his opinion as to the special callings to certain ways of life that some young men are supposed to have. Like ourselves he has no faith in these; he regards them as mere fancies which are liable to give way to other fancies with as little reason in them as they themselves had when they first began to exist. The persons who succeed best in professions are, he says, those who persevere as a matter of duty; they often feel their new pursuit to be unattractive enough in the beginning, but as they go on and acquire knowledge the case is altered, and then they become interested in what they are about. We believe this matter of likings and dislikings to be the cause of the wrecking of the fortunes of many young men, who if they entered on their work with earnestness would soon find pleasure where all seemed irksome. In Sir Benjamin's case his first teachings had a great effect in fixing his taste.

"During my first season in London, I attended Mr. Abernethy's lectures on Anatomy. He was an admirable teacher. He kept up our attention so that it never flagged, and that what he told us could not be forgotten. He did not tell us so much as some other lecturers; but what he did, he told us well. His lectures were full of original thought, of luminous and almost poetical illustration, the tedious details of descriptive anatomy being occasionally relieved by appropriate and amusing anecdotes, which, though they had been repeated over and over again, as one course succeeded another, were very agreeable to us new-comers. Like most of his pupils, I was led to look up to him as a being of a superior order; and I could conceive nothing better than to follow in his footsteps; and thus I was led to regard the department of the profession to which he belonged as that to which I should belong myself. Of this conclusion I have never since had reason to repent; and after an experience of fifty years, I am confirmed in the opinion that the pursuit of what is called pure surgery, such as it is in large cities, in connection with a hospital and a medical school, is more replete with interest, and, on the whole, more satisfactory, than any of the other branches into which the *ars medendi* is divided."

Among his earliest associates was Lawrence, who still survives him, and of whom he gives the following sketch:—

"From that time to the present, Lawrence and myself have been moving in parallel lines, he having had the largest share of private practice next to myself; and it may be regarded as somewhat to the credit of

both of us that there has never been any manifestation of jealousy between us. I have already mentioned that, when a young man, he had some faculties in great perfection, and he has them still, but little (as far as I can see) impaired by the addition of fifty years to his age. He has a great memory, and can readily recur to, and make use of, what he knows. He has considerable powers of conversation, but without obtruding himself to the exclusion of others, as is the case with too many of those who are reputed to be good talkers. What he says is full of happy illustrations, with, at times, a good deal of not ill-natured sarcasm. In public speaking, he is collected, has great command of language, and uses it correctly, but not equal to what he is in the ordinary intercourse of society. In writing his style is pure, free from all affectation, yet in general not sufficiently concise. His reading has been extensive: he is well acquainted with modern, and moderately so with the ancient, languages. His professional writings contain a vast deal of information, but it is more as to what he has taken from other authors than as to the results of his own experience and observation. That he is thoroughly acquainted with his profession cannot be doubted, for it would not have been possible for him otherwise to retain for so long a period the high place which he has occupied."

His eldest brother, who was preparing for the bar, was also studying in London at this time; he and Sir Benjamin lodged together, and they were thrown into the society of men older than themselves, and of refined and cultivated tastes, many of them already beginning to be known, and destined to become distinguished in their several ways. Through the influence of one of these, Dr. Maton, he was elected a member of the "Academical Society;" and even at this early period in his career he read papers on the benefits of metaphysical enquiries and on the mode of conducting scientific researches, showing that, though he was working hard at anatomy and chemistry, he found time for other studies; and it is interesting to observe that his earliest paper and the last essay he ever published were on the same subject—Psychological Enquiries.

During his second winter, by the advice of Dr. Baillie, he spent some time in a chemist's shop, to gain some knowledge of *materia medica* and the making-up of prescriptions; and in the spring of 1803 he first entered as a pupil, under Mr., afterwards Sir Everard, Home, at St George's Hospital.

"The commencement of my studies at the hospital was that of a completely new era in my life. Hitherto it is true that I had worked hard enough. With the exception of Lawrence, I doubt whether any one



of my acquaintance had been equally diligent. But it was rather as a matter of duty, or rather I ought to say of necessity, than because I felt any very great interest in what I was doing; and most willingly, if I could have afforded it, would I have turned my back on anatomy and returned to literary pursuits. A great change took place as soon as I became familiar with the business of the hospital.

“To those who really desire to learn, the wards of a hospital are soon found to be replete with interest. At first all is confusion. The nice distinction of symptoms on which the diagnosis of disease depends, why the pulse in one case indicates immediate danger, and in another none at all, why one patient recovers and another dies, why the same kind of treatment is successful in one instance and fails in another; these, and a multitude of other matters, are quite inexplicable to the young student. Everything is seen as it were through a mist. After no long time, however, the mist begins to clear away, and whoever has advanced thus far finds no difficulty afterwards. Every case is an interesting subject of enquiry. A great game is being played in which the stake is often neither more nor less than the life or death of a fellow-creature, and in which those among the students who devote themselves to their business perform a humble yet not unimportant part without any painful feeling of responsibility. Not many months elapsed before I became sensible of the good effect of these new studies, and of the wisdom of Dr. Baillie’s advice that I should make myself a tolerably complete anatomist before I commenced my attendance at the hospital; as I found that I was able to comprehend many things that were passing under my observation which I could never have properly comprehended otherwise, and in which those who were less prepared in this respect were little able to understand.

“During the summer of 1803 I never failed to pass the early part of the day in the wards of the hospital. In the afternoon I usually dined by myself at my lodgings in Knightsbridge, and in the evening read some Latin classics, and other books which formed my scanty library, or a novel from a small circulating library at Brompton, or walked in Kensington Gardens. As the season advanced, most of my friends left London. A few, however, remained, whom I met occasionally; among them was Dibdin, since known by his works on Bibliography, who at that time resided at Kensington, not very far from my lodgings at Knightsbridge, and with whom I occasionally wandered to hear the nightingales in the lane beyond Holland House.”

“During the summer of 1804, a friend of mine, of the name of Jeffreys, was house-surgeon of the hospital, and my intimacy with him enabled me to pursue my studies there with great advantage. He had more knowledge of his profession than most young men of his standing. In

the early part of the day, I was always with him in the wards ; and in the evening, we were generally together. It was from him that I first learned the importance of keeping written notes of cases, a practice which I continued ever afterwards. These notes I have carefully preserved. They now form many thick quarto volumes of manuscripts, to which (and even to the earliest of them) I not unfrequently refer with advantage, even at this advanced period of my professional life. My custom has been to take short notes at the bedside of the patients in the day, and to expand them, with the aid of my memory in the evening. Thus they became an exercise of the memory, and, instead of weakening, tended to strengthen that important faculty. After an experience of nearly fifty years, I am satisfied that no one can be well acquainted with his profession, either as a physician or surgeon, who has not studied it in that manner. It is only by these means that a case can be thoroughly and scientifically investigated, or that that minute and accurate knowledge of it can be obtained which is necessary to a right diagnosis. For one who is to occupy hereafter the situation of a consulting practitioner, to whom younger or less experienced persons will apply for assistance in cases of doubt or difficulty, it answers another purpose, as it enables him to express himself with greater facility, and especially to give written opinions with a degree of clearness and precision with which he could not give them otherwise. I have always, during the many years in which I was a teacher, and a hospital-surgeon, endeavoured to impress on the minds of my pupils the necessity of making and preserving such written records of their experience ; and I have often been pained to observe how small a proportion have followed the advice which I gave them. Some of them find a difficulty in doing so from the want of original education, and really not having a sufficient knowledge of the use of language even for this simple kind of literary composition ; others neglect it from mere idleness ; while the great mass of students, whose period of professional education is limited, are so occupied by the great (and, as I think, unnecessary) number of lectures which they are now required to attend, and in running from one class-room to another, that they really have neither the leisure nor the physical powers necessary for pursuing, in any efficient manner, the practical study of disease in the wards of the hospital."

Looking back at this period of his life, Sir Benjamin makes remarks on the value of lectures and methods of education too much in accordance with the views we have, on several occasions, given expression to in these pages to be passed over in silence:—

"I must not pass over this part of my life without noticing a very great advantage which I possessed during the period of my professional

education, compared with what I should have had if I had lived in these later times. No rules were then laid down as to the number of lectures which I was required to attend. The examination at the College of Surgeons was sufficiently good, as far as it went, but it was of a very simple and elementary kind. It was no more than a diligent student might pass without any special preparation for the purpose. The consequence was, that I was enabled to take my education very much upon myself; and I soon found that I could no how obtain so much useful knowledge as by a diligent attendance on the dissecting-room, and on the wards of the hospital. I cannot say that I neglected the use of books, but it was more in the way of reference and illustration than by a regular course of reading. I attended lectures on Anatomy, and, during one season, Dr. Crichton's lectures on the Practice of Physic, *Materia Medica*, and Chemistry, the latter especially with some advantage. During my first season in London, I had entered as a pupil to Mr. Abernethy's lectures on Surgery; but having at that time seen no surgical practice, I did not understand them, and soon ceased to attend them. I afterwards entered to some other lectures on Surgery, at the West-end of the town, but found that I learned nothing from them, so I ceased to attend there also. Mr. Home was accustomed to give an annual course of twelve surgical lectures gratuitously to the pupils of the hospital. These were excellent, and I attended them, year after year, with great advantage. Altogether, I do not suppose that I attended one-fourth of the number of lectures which the unfortunate students are now required to listen to under the direction of the constituted authorities. But I was acquiring knowledge in other ways, and much more substantial knowledge than can be acquired from such dull and hum-drum discourses as lectures usually are; and, which is better still, I had leisure to make my own observations, to think and reflect. Nor was this style of education peculiar to myself. I remember when Mr. Abernethy complained that Lawrence would not attend lectures. My friends and contemporaries, Jeffreys and Lawrence, took the same course; and so it had been with Nicolson, who was some few years in advance of us. I can easily conceive that, if I had been compelled to sit on the benches of a theatre four or five hours daily, or tempted to compete for prizes as students are, and to get crammed for various examinations, my position in life afterwards would have been very different from what it has been in reality."

He had now been house surgeon to St. George's Hospital, an office filled by one of the better informed pupils, and which may be held twelve months, but Sir Benjamin was induced to resign it after six months, by an offer of a demonstratorship of anatomy under Mr. Wilson; and he was employed at the same time by Home to assist him in his private operations, and to help to carry



out his inquiries in comparative anatomy; and about this time he became acquainted with Sir Joseph Banks, who proved a most valuable acquaintance.

Fortune hitherto so lavish in her favours, finding all her gifts so well taken advantage of, now determined to mark Sir Benjamin as her own, by bestowing one more important still, and accordingly in March, 1808, before he had completed his twenty-fifth year, he was elected assistant surgeon to St. George's Hospital; and as one of the surgeons was absent for some nine years serving with Wellington on the Peninsula, he had entire charge of his patients. His hospital duties now occupied a great part of his time. The custom at this period was for the surgeons to go round the wards on two days of the week only, not attending otherwise except when there were operations or severe accidents to be attended to; but Sir Benjamin and his colleague, Mr. Robert Keate, adopted the plan of attending daily and superintending everything in urgent cases, visiting several times a-day. Sir Benjamin moreover began to give clinical lectures, the first of the kind ever delivered in a London hospital. He also began now to lecture on surgery at the school; and very popular and well attended his lectures were.

Hitherto he had been living in lodgings, but he now took a house, 22, Sackville-street, placed his name on the door, and began to think seriously of private practice, obtaining in the following year from this source between £200 and £300. This with his other resources made him free from anxiety, and he was able in 1810 to engage with considerable interest in some physiological inquiries on his own account, the results of which formed the subjects of two papers communicated to the Royal Society, of which he had been elected a Fellow; one "On the Influence of the Brain on the Action of the Heart and the Generation of Animal Heat;" the other "On the Effects Produced by certain Vegetable Poisons." The first of these had formed the subject of the Croonian Lecture in 1810, and obtained for him the Copley gold medal. At this time he was only twenty-eight years of age; and he records, with much pride, that when the question of giving him the medal was discussed in the council, the only objection raised was that it had never been given to so young a man; to which Dr. Wollaston said, that he thought if he deserved the medal that was only an additional reason for giving it to him.

During this time his private practice was gradually increasing at the rate of £200 to £250 a-year; and he was continuing his



physiological investigations, and entering on his study of the diseases of joints, on which he communicated a paper to the Medico-Chirurgical Society in 1813. In 1812 changes in the school led to his giving up lecturing on anatomy; and, writing after the lapse of forty years, he refers to the familiarity with anatomy, derived from his teaching it during seven years, as having been of the greatest service to him.

In 1814 his health began to suffer from unceasing occupation of mind and body for so long a period, and partly from having been in London for ten years, and never breathing the air of the country for more than two or three days at a time, and then only at rare intervals; and his friends began to speculate as to his soon making a vacancy at St. George's Hospital; but a visit to the sea side soon restored him to health; and in 1816 he married the third daughter of Serjeant Sellon, then but nineteen years of age, he being thirty-three. At this time his professional income derived from fees and lectures amounted to £1,530, concerning which he makes the following rather *naïve* remarks:—

“I had previously saved sufficient money to refurnish and paint my house, and in other ways make it more fit than it had been before for the reception of a bride. I now, for the first time, had a carriage and pair of horses. In other respects, we made very little addition to my former establishment. As my wife had no fortune given her at the time of our marriage, nor indeed any except what had been settled on her after her father's and mother's deaths, and as my profession entailed some expenses on us, we were under the necessity of being careful as to our mode of living. My dear wife had no expensive habits, and we managed to make both ends meet at the end of the year. Still, I cannot but say that this was a period of considerable anxiety, when I felt for the first time that another individual as well as myself, and probably children hereafter, had to depend, not only on my professional character, but also on my bodily health. Fortunately, in the beginning of the following year there was a more manifest increase of my practice than there had ever been before. This kept my anxiety within bounds; still it was considerable, and was probably the cause of my having some return of the dyspeptic symptoms under which I had laboured formerly, and which continued to trouble me, from time to time, for the two or three following years.”

In a year or two, however, notwithstanding the recurrence of the dyspepsia from his former rashness, we find him incurring fresh expenses, but without any injurious effect on his health:—

“During the first two or three years after our marriage we continued to reside in the small house in Sackville Street in which I had resided previously. In the beginning of the year 1819, however, I took a house of greater pretensions in Savile Row, and we remained in it until we removed to a larger one in the same street, which we still inhabit. As my income had been steadily increasing, I felt myself to be guilty of no imprudence in making this change, and the event justified me in doing so, as my income in 1819 exceeded that of the previous year by more than £1,000. This increase may be in part attributed to the publication of the first edition of my work on ‘Diseases of the Joints,’ which had taken place in the previous year. Other circumstances, however, contributed to it. Although I was no more than thirty-six years of age, my name had been for several years before the public. Sir Astley Cooper, who had succeeded to the large practice of Mr. Cline and the smaller one of Sir Everard Home, too confident of his position, had already begun to lose some of the vast reputation which he had previously enjoyed. Some one else was wanted, and I was ready to fill the vacant place. From this time my practice steadily increased, so that almost every year made considerable additions to it. Hitherto my income had been little more than sufficient to meet my annual expenditure, but I now began to lay by a considerable portion of it; and finding that I had the prospect of providing for my family, and of acquiring in the course of no very long time a moderate independence, I was relieved of much of the anxiety which I had formerly experienced.”

Though Sir Benjamin appears to have thought Sir Astley Cooper's reputation declining at this time, it appears from the next extract we shall make that Lord Liverpool did not take the same view:—

“It was in the year 1821, and while I held the office of professor at the College of Surgeons, that I was first called on to attend the king, George IV., under the following circumstances. His majesty had one of the common encysted tumours which occur on the scalp, which was large enough to be troublesome to him. He showed it to Sir Everard Home, who advised him to have it removed by an operation. The king was anxious to undergo the operation. His majesty, however, expressed to Sir William Knighton that he wished the operation to be performed by myself, Sir Everard being, however, present, and Knighton was commissioned to make this communication to me. I cannot say that I derived any particular satisfaction from it, as I found that I had already obtained the patronage of the public, and was quite contented with it. In the meanwhile, however, the subject of the proposed operation was mentioned to Lord Liverpool, who was then prime minister. Lord

Liverpool represented to the king that it was a matter which might concern the public as well as himself, and urged that nothing should be done without Sir Astley (then Mr.) Cooper being first consulted, and that, if an operation was determined on, that Sir Astley should perform it. Sir Astley being at that time the most conspicuous person in his profession, I cannot doubt that Lord Liverpool's judgment was quite correct. Accordingly, Sir Everard Home, Sir Astley Cooper, and myself were summoned to Windsor; when, after examining the tumour, we agreed that nothing but an operation could be of any service, and that it should be performed when the king returned to London. Mr. Cline was consulted afterwards, who confirmed the opinion which we had given. Eventually the operation was performed by Sir Astley Cooper, in the presence of Sir Everard Home, Mr. Cline, Sir William Knighton, the king's physicians, Sir Henry Hallford, Sir Matthew Tierney, and myself, making indeed a very large assembly for so small a matter. After this attendance, Cooper was created a baronet, and Sir Everard Home was comforted by being appointed to the office of surgeon to Chelsea Hospital, vacated by the death of Mr. Thomas Keate, and by his son, who was then a very young lieutenant in the navy, being advanced rather prematurely to the rank of commander. From this time, when any surgical operation was required, the king, for some years, was in the habit of applying to Cooper; but on some special occasions I was summoned to meet him in consultation, though I held no actual appointment in the royal household until the year 1828, when, on Sir Astley having been appointed Serjeant-Surgeon, I was gazetted as surgeon to his majesty's person, in his place."

In 1834 Brodie was created a baronet, which served as a further incentive to exertion that he might be enabled to leave his successor an income more worthy of the title; but we must now hasten to conclude our sketch. We regret that we cannot give more extended notices of Sir Benjamin's criticisms on his contemporaries, which are full of interest and instruction, as showing the materials out of which successful practitioners are made; but for these we must refer to the autobiography itself, which has, we find, been published in a separate form. It closes with his resignation of the surgeoncy of St. George's Hospital in January, 1840. He held the office of assistant surgeon for fourteen years, and of surgeon for nearly eighteen. In resigning he was influenced by various motives; he wished to lessen the amount of his labours, and was unwilling to have it said of him, as he had heard it said of others, that he retained an office of such importance and responsibility, when either from age or indifference he had ceased to be fully equal to the

duties belonging to it; and lastly, with a feeling that we would commend to the notice of others, when he saw intelligent, and diligent, and otherwise deserving young men around, waiting their turn to succeed to the hospital appointments, it seemed to him that there was something selfish in standing longer in their way, when as far as his own merely wordly interests were concerned he had obtained all that he could desire.

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1. *The Second Step in Chemistry; or the Student's Guide to the Higher Branches of the Science.* By ROBERT GALLOWAY, F.C.S. With Illustrations on Wood. London: Churchill and Sons. 8vo. 1864. pp. 771.
2. *A Manual of Chemical Analysis, Qualitative and Quantitative, for the Use of Students.* By HENRY M. NOAD, F.R.S. London: Lovell Reeve, and Co. 8vo. 1864. pp. 663.

ALTHOUGH thus grouped together, and to a hasty glance upon similar subjects, these two works, both excellent of their kind, are of widely different scope, and aim at discharging offices of very dissimilar character indeed in the students chemical education. In the following notice we shall endeavour to point out to our readers what kind and amount of assistance they may expect to derive from either or both of them.

The student who has already mastered the elementary details of chemistry, and who is anxious still further to extend his knowledge, will find a rich treat awaiting him in Mr. Galloway's work; but it is as well for him to know, that without a tolerably accurate knowledge of the elementary facts in chemistry to attempt the perusal of this work would be indeed utter loss of time; possessed of this knowledge, however, we know no work in which he will find a fuller, and, at the same time, a plainer account of the more advanced departments of chemical science than in the work before us. In it will be found, fully recorded, the labours of Sir B. Brodie, the illustrious son of an illustrious father; Odling, Gibbs, Gerhardt, Laurent, Williamson, Hofmann, Bunsen, Kirchhoff, Wurtz, Playfair, Kolbe, Frankland, &c.—labours of rare value in themselves, tending as they do to revolutionize, in a few years, our present ideas upon various subjects in chemistry—but frequently inaccessible to the majority of students in their original form,



inasmuch as they are scattered over the world of literature in monographs, periodicals, &c.; many of them, also, being written in languages with which, probably, they are not familiar. Were this the extent of Mr. Galloway's achievements, it would be sufficient to secure for his work a high meed of praise as a laborious collection of the theories and facts of these distinguished investigators in the higher walks of chemical science; but, in addition, he gives us the results of his own experience, and also introduces into each chapter, after the enunciation of each principle, *exercises* by which the student is enabled to test whether he has mastered the details which he has just been reading, and by which excellent plan his mental powers are stimulated to healthy activity. If any system can ever eradicate that destructive and withering system of medical education of modern growth—*cramming*—it is this. By it only can the student ever arrive at his intellectual legs, and be able to walk alone, not dependent on the pernicious plan of mental spoon-feeding that is the bane of our present students, and which, in years to come, they will so bitterly regret. Although apparently foreign to the subject we have in hands, yet these considerations are forced upon our consideration by the excellence of the course pursued by Dr. Galloway in the work before us, and which, perhaps, will be best understood by our readers having submitted for their perusal an example. Such may be taken at hap-hazard from any portion of his work, for almost in every page of it is this excellent plan fully carried out.

“1. SPECIFIC GRAVITY OF GASES AND VAPOURS.—Gases and Vapours differ in their densities, or specific weights.

“2. Atmospheric air, at 60° F., and the barometer standing at 30 inches, is employed as the standard of comparison for gases and vapours. One hundred cubic inches of air weigh, according to the latest researches by Regnault, 30·935 grains *at this temperature and pressure*. Air is therefore about 814 times lighter than water, as 100 cubic inches of water weigh 25246·0 grains.

“3. *If the specific gravity of a gas or vapour be known, the absolute weight of a given volume of it can be determined.* To accomplish this, we have simply to multiply the weight of an equal volume of air by the specific weight of the gas or vapour; the product will be the weight of the volume, at the standard temperature and pressure of the gas or vapour.

“Example.—What is the weight of 100 cubic inches of hydrogen, its specific gravity being ·0694?

“ $30\cdot935 \times \cdot0694 = 2\cdot147$  grains weight of 100 cub. in. of H.

*"If it be desired to find the volume of a given weight of any gas or vapour, the weight of some volume (say a cubic inch) of the gas or vapour must first be ascertained by the preceding rule; then the given weight must be divided by the weight of the cubic inch; the quotient will be the volume in cubic inches of the given weight of the gas, at the standard temperature and pressure.*

*"Example.—What is the volume of 2·147 grains of hydrogen, its sp. gr. being 0·0694?*

$$\frac{2\cdot147}{\cdot02147}=100 \text{ cubic inches.}$$

**"EXERCISES.**

*"1. What is the weight of a cubic inch of oxygen, its sp. gr. being 1·1057?*

*"2. What is the weight of a cubic inch of nitrogen, its sp. gr. being 0·9713?*

*"3. What is the weight of a cubic inch of carbonic acid, its sp. gr. being 1·529?*

*"4. What is the weight of a cubic inch of gaseous ammonia (NH<sub>3</sub>), its sp. gr. being 0·59?*

*"5. What is the volume of 54 grains of chlorine, its sp. gr. being 2·44?*

*"6. What is the volume of 45 grains of carbonic oxide, its sp. gr. being 0·967?*

*"7. How many cubic inches of oxygen would be obtained from 100 grains of chlorate of potash?*

And in the Appendix II. this system is still further worked out; a series of questions originally given at the several examining boards is introduced, with references to such portions of the work as will enable the student to solve them. In all this a direct appeal is made to the exercise on his own part of the student's reasoning powers; and, at the same time, he has afforded him an amount of assistance not incompatible with a healthy development of his intellectual faculties. We need scarcely add that we most heartily wish Mr. Galloway's valuable and masterly treatise all the success it so honestly deserves.

As we have already intimated the two works, the titles of which head this article, though apparently on the same subject, materially differ in the object at which they aim. So far we have pointed out the distinctive features of Mr. Galloway's work; it now remains for us to notice those of Dr. Noad's. We confess that when we first saw it our impression was that in the presence of the existing

treatises upon the same subject (and here we, in an especial manner, allude to the masterly work on chemical analysis, by Fresenius), its appearance was uncalled for; but, upon examination, we found our first impressions to be wrong, and we gratefully acknowledge that Dr. Noad's chemical analysis will find a place for itself in our scientific literature. It is divided into twelve chapters, the first two of which are occupied with the consideration of what may be termed the prolegomena of chemical manipulation, the various apparatus and reagents required for qualitative and quantitative analysis being described in terse yet accurate terms. Chapter three is devoted to the chemical characters of the several metallic oxides; chapter four to those of the principal organic and inorganic acids; chapter five to those of the principal poisonous alkaloids; whilst chapter six is occupied with the consideration of the general principles upon which a qualitative analysis should be conducted. Chapters seven and eight are devoted to quantitative analysis; chapter nine to the analysis of organic bodies, chapter ten of mineral waters, chapter eleven of soils, and chapter twelve of the ashes of vegetable and animal substances. The work concludes with an Appendix containing several important tables, the first of which is of special value, inasmuch as it enables us to arrive at the exact amount of any elementary substance present in a given weight of any compound into which it enters. The application of this table will be understood by the following extract:—

“The application of this Table, and its convenience in quantitative analysis, may be illustrated by the following example:—

“Suppose as the result of an experiment to determine the amount of *sulphur* in a certain substance, 13·75 grains of sulphate of baryta have been obtained. On turning to the Table we find, under the head of “required *sulphur*” found, “*sulphate of baryta*,” a series of figures arranged in nine columns, those in the first column indicating the amount of sulphur in a *unit* of sulphate of baryta, and those in the other columns the multiples of that number by 2, 3, 4, 5, 6, 7, and 9. In the case we are considering, we require to learn the amount of sulphur in 13·75 grains of sulphate of baryta: we turn to Column 1, where we find the number 0·13734; we put down this number, shifting the decimal point one figure further to the *right*; we next turn to column 3, where we find the number, 0·41202; we place this number under the former, preserving the decimal point unaltered; the next number is 7, under which column we find the number 0·96138; we place this number under the last, shifting the decimal point one figure further to the *left*; in like manner the

last number being 4, we write down the number found under that column, viz., 0·6870, shifting the decimal point two figures further to the left; the whole will consequently stand thus:—

“ 1·373400

0·412020

0·096138

0·006867

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1·888425

13·75 grains of sulphate of baryta contain, therefore, 1·888 grains of sulphur.”

So far we have only given our readers an insight into the matters to be found in this volume. It now remains for us to add, that whoever consults its pages will find in them full and clear directions for undertaking that most important duty, a chemical analysis. The work is evidently written up to the present stage of chemical knowledge, and is, in addition, enriched with well-drawn woodcuts. As already more than once stated these two works apply themselves to two widely different branches of the same science, and both merit our best commendation.

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*Register of the King and Queen's College of Physicians in Ireland; to which is appended a Roll of the Presidents and Fellows from 1654 to 1865—Corrected to May 1st, 1865.*

WE congratulate the College of Physicians on the appearance of this the most full and complete register ever published by them. It extends over 90 pages of large 8vo, and comprises a concise historical introduction, register of officers, of fellows, of honorary fellows, of licentiates, of licentiates in midwifery, as well as a general alphabetical register of the names, qualifications, and addresses of all ranks; and a roll of the presidents and fellows from the earliest period to the present time. A good deal of the historical introduction appears to be an abstract of a memoir of Dr. Stearne, which appeared in our last number; and much attention seems to have been given to furnishing the profession with what has been long wanted—a *resumé* of the existing legal enactments which concern the profession of physic in Ireland.



There is also in it a great deal of curious and almost unknown information respecting this college, which has on its roll many illustrious names.

As compared with other medical bodies, the corporation seems aristocratic, as it consists of forty-four fellows, eleven of whom are non-resident. Among the forty-four, however, are numbered our most eminent physicians. The government of this college is not virtually delegated to representatives, as in the cases of our more democratic corporations, for we read thus in the introduction, p. 11:—"The fellows alone constitute the corporation and govern the college, without the intervention of a board, council, committee, or senate."

The honorary fellows number thirty-three; the licentiates 597; and the licentiates in midwifery 170. Many of the licentiates are in all branches of the public service; and the college has numerous representatives on the continent, and in every part of the British dominions. An Irish physician may well desire to see his name in so honourable a roll.

From the appendix (roll of fellows, &c., 1654, 1865) we learn that the editor intends to extend it into short biographies. We suppose some of these will shortly follow the memoir of Dr. Stearne, the first fellow; but we trust it will be long ere the editor has an opportunity of giving us a biography of the last fellow—the Rev. Professor Haughton.

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*On Combined External and Internal Version.* By J. BRAXTON HICKS, M.D., &c. London: Longmans. 1864. 8vo. pp. 72.

THE object of this *brochure* is to recommend the general adoption of a mode of performing the operation of version, devised by the author, by introducing one or two fingers of the one hand, within the os uteri; and with them tilting the child in whatever direction it may be desired to turn it, and placing the other hand on the mother's abdomen, and with it assisting the movement of the child. This, as is pointed out by the author himself, is a combination of the method of external version of Wigand, with the method of Lee and others, who endeavour to perform the operation by introducing only two fingers into the os, and while it obviates the necessity for introducing the whole hand into the uterus, and

thereby lessens very considerably both the pain and risk of the operation, it facilitates its performance, and allows of its being done successfully where neither of the other modes would be practicable. Dr. Hicks claims for it that it permits of cephalic version being performed with almost as much certainty as podalic, but we must allow him to describe, in his own words, his mode of operating; and first as to *podalic version*, the head being the presenting part:—

“I will now proceed to describe the mode by which I effect podalic version. (See cases 1—16). We will suppose a case where everything is natural; the os uteri dilated to admit one or two fingers, membranes perfect, and the face towards the right side.

“The patient may be placed in the ordinary obstetric position.

“Having lubricated my left hand, I introduce it as far into the vagina as is necessary in order to reach a finger's length within the cervix—sometimes it requires the whole hand, sometimes three or four fingers will be sufficient in the vagina. Having clearly made out the head and its direction, whether to one side or the other of the os uteri, I place my right hand on the abdomen of the patient towards the fundus; I then endeavour to make out the breech, which is seldom a difficult matter. The external hand then presses gently but firmly the breech to the right side; as it recedes, so the hand follows it either by gentle palpitation, or by a kind of gliding movement over the integuments, while at the same time the other hand pushes up the head in the opposite direction, so as to raise it above the brim.

“It may here be mentioned that when the head has descended a considerable distance into the pelvic cavity or more than half way through the os uteri, it is scarcely possible to lift it above brim, especially if the uterus be active.

“When the breech has arrived at about the transverse diameter of uterus, the head will have cleared the brim, and the shoulder will be opposite the os. That is pushed on in like manner as the head, and after a little further depression of the breech from the outside, the knee touches the finger and can be hooked down by it. It very frequently happens when the membranes are perfect, that as soon as the shoulder is felt, the breech and foot come to the os in a moment, in consequence of the tendency of the uterus to bring the long axis of the child coincident with that of its own.

“Should it therefore be difficult to hook down the knee, depress the breech still more, and it will be almost always the case that the foot will be at hand.

“It will sometimes render turning more easy if, as soon as the head is above the brim, we pass the outside hand beneath it, and push it up

from the outside alternately with the depression of the breech. All this can generally be performed in a much less time than I have taken to describe it, although in some it requires gentle, firm, and steady perseverance, with such a supply of patience as is always demanded in obstetric operations."

In performing this operation ourselves, we have preferred to introduce the fingers of the *right* hand into the uterus, instead of the *left*, as directed by Dr. Hicks, because then, with the patient lying on her left side, the left hand can be placed on her abdomen without being carried up between the thighs, as should be done in Dr. Hicks' method, and this is not only less annoying to the patient, and less irksome to the operator, but allows him to have greater freedom of action.

In commencing the operation, the first step is of course to ascertain the position of the child in utero, so that it may be turned in the direction that will most easily accomplish the object in view.

"In regard to *transverse* presentations, it has already been pointed out (p. 8) that in their early stage it is really seldom necessary to employ more than one or two fingers within the os, in order to hook down the knee; should that, however, not be the case, then the best manner of proceeding, if we determine upon *podalic* version, will be simply to depress the breech from the exterior, as has already been described; if further assistance be required to produce that change, to push on the elbow as before mentioned, and the head up from the outside. This latter procedure is seldom required while the membranes are perfect.

"It may here be remarked that the use of the external hand in lifting the head from just above the pubes towards the fundus is very great, and tends to facilitate remarkably the operation of turning, even in those cases in which the old plan is used. In those cases where the descent of the feet and breech is found difficult, the head, easily recognised from the outside, can be pressed upwards if necessary to the very fundus. When this is accomplished the difficulty is generally over."

In cases of transverse presentation, where the head lies nearer the os than the breech, Wigand recommends cephalic version, by external manipulation, and we shall see that Dr. Hicks finds this operation of comparatively easy accomplishment by his method of combined external and internal action; where, however, the arm is prolapsed outside the vulva, and the thorax jammed into the cavity of the pelvis, while the head and breech are in the uterus, and the uterus tightly clamped round the body of the child, Dr.

Hicks thinks it advisable to introduce the hand into the uterus in the ordinary manner, but we presume he means this to apply to such of these cases only as the operation of version may be attempted in, instead of evisceration.

The following are Dr. Hicks' directions for *cephalic* version:—

“We will suppose first of all a case where the uterus is not active, the liquor amnii not escaped, or only recently so, where the foetal hand has not passed the os.

“Introduce the left hand into vagina as in podalic version; place the right hand on the outside of abdomen, in order to make out the position of foetus, and the direction of the head and feet. Should the shoulder, for instance, present, then push it with one or two fingers through the cervix in the direction of the feet. At the same time pressure by the outer hand should be exerted on the cephalic end of the child. This will bring down the head close to the os; then let the head be received upon the tips of the inside fingers.

“The head will play like a ball between the two hands; it will be under their command, and can be placed in almost any part at will. Let the head then be placed over the os, taking care to rectify any tendency to face presentation. It is as well, if the breech will not rise to the fundus readily after the head is fairly in the os, to withdraw the hand from the vagina, and with it press up the breech from the exterior. The hand which is retaining gently the head from the outside should continue there for some little time, till the pains have ensured the retention of the child in its new position, by the adaptation of the uterine walls to its form.

“Should the membranes be perfect it is advisable to rupture them as soon as the head is at the os uteri; during their flow and after, the head will move easily into its proper position.

“The ease with which cephalic version was effected by these means in one of the cases to be mentioned was such, that only half a minute was required. But if any unseen circumstance oppose difficulty to cephalic version, it will still remain open to us to adopt podalic in the same case. All that is required is to place the hand on the opposite end of the child, and depress it as has been already shown, pushing the head and shoulder from within; while the fingers are ready to hook down a knee or foot. This took place in one of the cases about to be recited. (Case 19.)

“From the opportunities I have had of judging I may lay it down as a good rule, that, in neck and transverse presentations, where we have no reason to hurry, and where all things seem otherwise favourable, we should first of all endeavour to induce cephalic presentation; and then,



should there be any difficulty in accomplishing it, to change our plans to podalic, which merely requires, as just shown, the action to be reversed.

"If in these malpresentations the foetal hand protrude, it is even then quite possible to induce cephalic presentation, provided the thorax has not yet descended (as already mentioned. See Case 20). We must first, in this case, carefully replace the presenting hand into uterus, and then proceed as just indicated.

"The best mode of replacing the arm is first to bend the forearm upon the arm in the vagina; the foetal hand will then be close to the breast of the foetus; by pushing gently the elbow the hand will pass over the front of the chest and thus be effectually reduced, making the case much more simple.

"The advantages which accrue from the institution of cephalic presentation, in a case of labour without any other complication than that of malposition of the foetus, is admitted by, I may say, all prudent practitioners, and the readiness with which it can in many such cases be effected by the above plan induces me strongly to recommend its adoption. Of course I mean with an ample pelvis, or small head, for in many cases transverse presentation is the result of malformed brim."

Dr. Hicks considers his method to be of special service in *malpresentations*; which may be rectified by it at an early period. In *convulsions*; where the uterus may be emptied of its contents, long before this could be safely done by other methods. In cases of *extreme depression*; where it is necessary to turn, this, he says, may be done with much less shock than when the whole hand has to be carried to the fundus. In *placenta previa*; where the os will not admit more than a finger or two, the bleeding may, he says, be controlled by laying hold of a leg, and drawing down the child as the os dilates, so as to keep it plugged, after the following method:—

"If I can feel the membranes and find the head presenting, I then proceed as in an ordinary case before described. When the foot has arrived at the os, I rupture the membranes, draw the foot and leg through as far as they can descend without force. By exerting gentle traction with merely the weight of the arm, we have a firm plug on the bleeding part. I retain hold of the leg in this manner; and thus as the os expands, the leg by its conical form makes an excellent plug, as does also the breech when it has entered the os.

"When this point is secured, most valuable time is at our command, and our endeavours should be devoted to restore the vital powers, weakened and in some cases almost annihilated by the previous loss. In

extreme cases—I am sure most of my readers will agree with me—the worth of this period can be scarcely over estimated.

“Labour pains are now waited for; and the case treated as is proper in breech and footling cases.”

In Chapter V., Dr. Hicks summarizes the advantages and difficulties of his method of operating, and he concludes by giving cases illustrative of its performance in placenta previa, accidental hemorrhage, convulsions, coarctation of the brim, prolapse of the funis, and transverse presentations, in two of which latter cephalic version was performed.

In conclusion, we can only say, we have given Dr. Hicks' method a full trial, and we cordially endorse his recommendations of it.

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*Medical Errors. Fallacies Connected with the Application of the Inductive Method of Reasoning to the Science of Medicine.* By A. W. BARCLAY, Cantab. and Edin., Fell. R. Coll. Phys.; Phys. to St. George's Hospital, &c., &c.

DR. BARCLAY read the Lumleian Lectures, for 1864, before the Royal College of Physicians of London; and it seems that the present volume embodies the lecturer's views; for we thus read in the Preface:—"The lectures are now published just as they were delivered, with the addition, only, of certain portions which were suppressed at the time, so as to occupy no more than the three hours allowed me."

Beyond this statement, and the announcement that the typography of this work extends over 123 pages of small 8vo, we regret we cannot give our readers any better description of the book as a whole; for Dr. Barclay has not thought proper to divide it into lectures, or chapters, or component parts of any kind; nor to give a table of contents, nor an index. This omission we hold to be a grievous defect in a book of any kind, but much more when the particular book contains lectures, and lectures on so very dry and uninviting a subject as medical logic. In the opening portion of this volume Dr. Barclay undertakes to show "that although much behind what are called the exact sciences, still the principles of medicine are largely based on true and legitimate inductions;" and he asserts "that all deductions ought to be based upon principles

first arrived at by the inductive method." He states that the first part of the process, in the construction of an argument based on the inductive method, consists in the collection of particular facts which agree in some one or more points; their harmony forming the groundwork of the inference which is afterwards drawn. The next step is to frame an hypothesis to link together, or to explain the phenomena; and this must, at least, possess the character of embodying the general fact observed in each of the particular instances. The final portion of the reasoning process is to apply to other cases, by way of inference, a law which has been already established by induction.

"An induction," he observes, "is only complete when it establishes a law of causation." To prove this he gives instances of induction, true and false. Vaccination he regards as an instance of true induction; and having described (p. 67) the process by which Jenner arrived at his conclusion; he adds:—"When, therefore, he proceeded to make the experiments of first inserting the vaccine virus, and producing the disorder known as cow-pox, and after a short interval inoculating the same individual with small-pox matter, and when he had observed that in such circumstances no effect was produced by the inoculation, his induction at once established a law by which future generations have been so greatly benefited. Occasional failures cannot overturn the force of his argument, though they may prove that in some individuals the protective influence is speedily exhausted, or that in others it only serves to modify the disease when it occurs."

The practice of syphilization, on the other hand, he affirms, has none of the elements of true induction. The hypothesis that it acts as a preventive, was not based on ascertained cases, and the theory that fresh infection eradicates an old taint, was opposed to all experience. The use of belladonna in scarlatina, Dr. Barclay also regards as an instance of false induction, and proves his case at such length as to preclude even a sketch of it here.

In a former number of this Journal<sup>a</sup> we had occasion to review Dr. Laycock's work on *Medical Observation and Research*. Dr. Barclay refers to the labours of Dr. Laycock, from whose views he largely dissents. He considers that Dr. Laycock "has unintentionally obscured what he designates 'the purely inductive method of research,' by importing into his consideration of it much

<sup>a</sup> May, 1864.

of the deductive, or *à priori*, system of reasoning. . . . He explicitly states that the first step is the assumption of a theory, whereas the first real step is the ascertaining that we have facts to deal with, and a knowledge of their general bearing." To our mind Dr. Barclay is not only theoretically but practically right here. We all recollect that some years ago it was the orthodox theory, that there were certain conditions of disease, associated with what is called inflammatory action, in which blood-letting aided in the recovery of the patient. Whether this conclusion were right or wrong we shall not here discuss, but just remark that the assumptions made to give the character of a generalization to this supposed induction were of such a kind as to render conclusions drawn from them, whether true or false, quite untrustworthy. The blood drawn in such cases was observed to have the buffy coat, and hence it was concluded that blood-letting was useful because it withdrew from the circulation the morbid excess of fibrin. All this while it was forgotten that in withdrawing the fibrin most of the blood was also withdrawn, and the patient must needs die that the disease might be cured.

How many were thus bled to death none can say; but we all know that the reaction is now so great that scarcely any one dares to use a lancet, whatever he may think of the propriety of doing so.

Dr. Barclay draws a clear, but often ignored, distinction between medical statistics or averages, and true induction. This distinction may be thus shortly stated: an induction points out a relation of cause and effect, which can only be set aside by the interference of some more powerful and more universal cause; while an average, even when calculated on a large scale, is only a calculation of chances which gives no insight into the true relation subsisting between them.

It is scarcely necessary to show our readers that this is one of our most common medical fallacies; even in our most esteemed professional treatises numbers of cases are given without any indication of a direct relation of cause and effect. To prove this point Dr. Barclay instances statistics and inductions respecting rheumatic fever. Most persons recover from it, save when the heart becomes affected, and even then the deaths are comparatively few. We therefore learn almost nothing from the records of even a hundred cases of this disease. No matter what the treatment may have been, and for all the selected cases show, the result might have been the same if nothing but cold water had been given.



Dr. Barclay discusses, at considerable length, the attempt recently made by the British Medical Association to obtain records of the results of treatment in certain specified diseases. He enlarges on Bouillaud's treatment of rheumatic fever by the copious and repeated abstraction of blood, and declares "it required no more than the evidence of his own cases to show that his application of it was most pernicious." On Dr. Hughes Bennett's connexion with this therapeutic inquiry he largely comments, and shows that if statistics be taken as the basis of medical argument the Edinburgh professor has not much to show; since it appears from the army statistics that 16,000 cases of pneumonia gave a death rate of only thirty-nine per thousand during twenty years, when moderate bleeding was the practice, while Dr. Bennett's death rate, as compared with this, is forty-three. The treatment of tape-worm by the oil of male fern, the treatment of non-syphilitic psoriasis, and the treatment of scarlatina are discussed as to their bearings on the question of statistical investigations and the method of averages.

To refer to these slightly in detail:—On the treatment of tape-worm by the oil of male fern, Dr. Barclay observes:—"No inductive argument is, in my opinion, more conclusive in the matter of therapeutics than this, viz., that when a tape worm is present in the alimentary canal, certain remedies, of which oil of male fern is one, affect the parasite so as to cause it to let go its hold, and this is followed in due time by its expulsion, and the cessation of the symptoms dependent on its presence. Half-a-dozen instances in which the effect distinctly and rapidly follows the cause, are just as good as a thousand for the purposes of an induction, which serves to establish a definite law of this kind."

Respecting the treatment of non-syphilitic psoriasis, he observes, that it holds a place somewhat intermediate between that of acute pneumonia and tape-worm, and says:—"While it does not present the same degree of vagueness as that which has reference to the treatment of acute pneumonia, it has less of certainty than the treatment of tape-worm, and less pretension to be ranked as an induction." The treatment of scarlatina, he considers, should stand on the same ground as that of acute pneumonia, "except that it is made only in regard to a small number of remedies, and therefore the series need not be quite so long in order to obtain trustworthy results."

Dr. Barclay shows, we think, that from a large number of case

trustworthy averages may be obtained, and that from these, under proper restrictions, correct inferences may, occasionally, be drawn; but that this does not give collections of cases the character of inductions. The distinctive character of an induction consists in the discovery of some law which will stand the test of experiment in every case, except where we can show its failure to be due to the interference of some higher law.

To define the term "law," as applied to medical research, occupies no small share of Dr. Barclay's attention. He defines it to be "an expression of the mode in which any given cause operates to produce a certain effect," and considers that a large proportion of medical fallacies arise out of a supposed application of the inductive method of reasoning. The processes by which a correct result may be arrived at, and errors avoided he does not enter into, although, in our opinion, he, perhaps, might have done so, instead of referring his readers to *Mill's System of Logic*, where in Book III., Chap. VIII., we may leave them—only wishing that they may "read, mark, learn, and inwardly digest;" and it will require all these processes to come to the bottom of so deep a subject.

We have already referred to Dr. Barclay's critical review of the medical fallacies contained in Bouillaud's treatment of acute rheumatism; we now proceed to note, *passim*, that he discusses the fallacy involved in the administration of sarracenia in small-pox, the administration of the hypophosphites in phthisis pulmonalis, as well as the use of podophyllin and other remedial agents. One most important part we cannot lightly pass over. He looks on the late Dr. Todd's stimulating treatment of acute diseases as fallacious, because no experiments are given to show whether alcohol acted as a stimulus to the nervous system or as a general sustainer of life. Now we have looked over Dr. Todd's sixteenth lecture (in the large edition) "On the Therapeutical Action of Alcohol," and we think this statement of Dr. Barclay is scarcely correct. Dr. Todd does what Dr. Barclay does not—he defines the proper meaning of the term "stimulus," and the case numbered 84, in the same lecture, is most certainly an experiment of the most valuable kind. Had this work of Dr. Barclay's no other merit than that of directing attention to the absence of all connexion of cause and effect in the reasonings of many medical men we should gladly recommend its perusal to our brethren.

As a preliminary step, however, we think there is great occasion for avoiding two logical fallacies which we observe in several

lucubrations of our own day—the argument called *petitio principii*, or begging the question; and that so often appended to a record of treatment in a single case—*argumentum à particulari ad universale* “quod (say all the schoolmen) non valet.”

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1. *Tabeller over Folkmængden i Christiania den 31te December, 1863, samt over de i Tidsrummet, 1860–1863, indtrufne Dødsfald.* Udgivne efter Foranstaltning af Christiania Sundheds-commission.

*Tables of the Population of Christiania on the 31st December, 1863, and of the Deaths which occurred during the period 1860–1863.* Published by direction of the Sanitary Committee of Christiania.

2. *Norges officielle Statistik. Udgiven i Aaret, 1863. C. No. 4. Beretning om Sundhedstilstanden og Medicinalforholdene i Norge i Aaret 1861.* Udgiven af Departementet for det Indre.

*Official Statistics of Norway. Published in the year 1863. C. No. 4. Report on the Sanitary Condition and Medical Arrangements in Norway for the Year 1861.* Published by the Department for the Interior.

3. *Norges officielle Statistik. Udgiven i Aaret 1864. C. No. 5. Tabeller over de Spedalske i Norge i Aaret 1863, samt Aarsberetning for samme Aar til Departementet for det Indre fra Overlægen for den Spedalske Sygdom.*

*Official Statistics of Norway, published in the Year 1864. C. No. 5. Tables of the Spedalskic in Norway in the Year 1863, and Annual Report, for the same Year, from the Physician-in-Chief for the Spedalskic Disease to the Department for the Interior.*

4. *Generalberetning fra Gaustad Sindssygeasyl for Aaret 1856.* Ved OLE SANDBERG, Direktör. Christiania.

General Report of the Gaustad Asylum for the Insane, for the Year 1856. By OLE SANDBERG, Director. (Similar reports are sent for the years 1858, 1859, 1860, 1862, and 1863.)

5. *Index Scholarum in Universitate Regia Fredericiana centesimo secundo ejus Semestri Anno MDCCCLXIV. ab A.D., XVII. Kalendas Februarias Habendarum.* Christiania, 1864.

6. *Index Scholarum in Universitate Regia Fredericana centesimo tertio ejus Semestri Anno MDCCCLXIV. ab Augusto Mense Ineunte Habendarum.* Christiania, 1864.

7. *Om det syphilitiske Virus. Til Besvarelse af Priisopgaven: "Ere de Sygdomsformer, som vi hidtil have indbefattet under Benævnelsen Syphilis, frembragte ved eet Virus eller ved flere forskjellige?"* Af L. BIDENKAP. (Belønnet med Hs. Mj. Kongens Guldmedaille.) Aftrykt fra *Norsk Magazin for Lægevidenskaben*. Christiania, 1863. 8vo., pp. 126.

*On the Syphilitic Virus. In Answer to the Prize-question: "Are the morbid forms, which we have hitherto comprised under the denomination Syphilis, produced by a single virus, or by many different poisons?"* By L. BIDENKAP. (Awarded the Gold Medal of his Majesty the King.) Reprinted from the *Norwegian Magazine of Medical Science*.

8. *Iagttagelser under en Epidemie af Diphtheritis faucium.* Ved S. HÖEGH, Districtslæge i Vestre Söndmöre.

*Observations during an Epidemic of Diphtheria.* By S. HÖEGH, District Physician, &c.

9. *Det Kongelige Norske Frederiks Universitets Aarsberetning for Aaret 1862.* (Meddeelt of Universitetssecretairen.)

*Annual Report of the Royal Norwegian Frederiks University, for the Year 1862.* (Communicated by the Secretary to the University.)

THE above catalogue represents a rather heterogeneous collection of works which we have just received from Norway. The space at our disposal will be insufficient to enable us to do more than give our readers, very briefly, a general idea of the contents of each publication.

1. From the *Tables of the Population of Christiania on the 31st December, 1863*, it would appear that this amounted to 55,125, of whom 27,142 were males, and 27,983 were females; 12,675, or 23 per cent. of the entire population, being under ten years of age, while 42,450, or 77 per cent., were above that age. The proportion over seventy years of age was 1·48 per cent. The mortality for 1863 was 23·32 per 1,000. The calculated mortality for the last five years was 25·59 per 1,000; but a remark is appended that the



city was during that period severely scourged by epidemic diseases, some of which contributed not a little to increase the mortality.

2. With the second "blue" book on our list we are saved some trouble by the announcement on the cover, that "a statistical *résumé* is to be found at page iv." On turning to the latter we find, in fact, the following abstract in the French language, a translation of which will probably place our readers in possession of sufficient information respecting the subjects treated of in the volume:—

"During the year 1861 the general sanitary condition, and especially that of the two most northern dioceses of the kingdom, has been less favourable. Nevertheless the pathological constitution has not been of a very decided character. As to the epidemic diseases which have chiefly contributed to produce this unsatisfactory state, we would mention—the diseases of children, and particularly measles, which prevailed throughout almost the whole kingdom; diphtheria, which extended principally through the dioceses of Trondhjem and Tromsö, and through a great part of that of Christiania, and which must be considered as the most malignant disease of the year; as well as hooping cough, which likewise has been more than usually prevalent. Catarrhal and inflammatory diseases have also been a little more frequently met with than during the preceding years, while typhoid fever has kept pretty much within its ordinary limits.

"In 1861 the mortality has been rather greater than usual; calculated according to the estimated population of the year, or 1,635,617 individuals, it has amounted to 19·2 per thousand (not including the still-born; including the latter the mortality was 20·5 in 1,000). The corresponding numbers of the preceding year were 16·9 and 18·4. The number of deaths was 31,471; that of the still-born was 2,132. The number of births exceeded that of deaths by 18,053, being a diminution of 7,623 from the excess of the preceding year. Of 8,938 deaths, of which medical men have stated the causes, the following diseases have led to the greatest numbers:—Diphtheria and croup, 1,562; phthisis and pulmonary tubercles, 1,144; measles, 695; pneumonia, 551; typhoid fever, 506; decrepitude, 425; cerebral meningitis, 258; dropsy, 252; spédalskhed, 237; acute bronchitis, 220; hooping-cough, 217; cancerous diseases, 205; cerebral apoplexy, 186; convulsions, 159. In addition, 891 individuals perished by accident, the majority having lost their lives by drowning.

"The number of hospitals was 42; in the 34 ordinary hospitals 7,511 patients were under treatment, the mortality being 1 in 12·2. In seven asylums for the insane there were 794 inmates.

"The medical administration of the country was divided into 116 civil districts. The total number of authorised medical men was 331. Vaccination was performed upon 39,242 persons, whether by medical men or by assistant-vaccinators, the number of the latter being 635. There were 483 midwives who had passed their examination.

"The number of pharmacies was 54."

3. With respect to this volume also we need scarcely go beyond the summary given at page iii., that "the number of *spedalskie* individuals ascertained to exist in Norway, at the close of the year 1863, was 2,155, of whom 747 were in the hospitals."

4. The first annual Report of the Gaustad Asylum for the Insane, that, namely, for the year 1856, opens with a full description of the institution, illustrated with ground plans; and it and the reports of subsequent years contain much valuable information respecting the general care and medical treatment of the inmates. We must, however, content ourselves with quoting a few statistics from the latest of the reports, that issued for the year 1863. From it we find that the number of patients in the asylum during the year was 439, of whom 189, or 99 men and 90 women, were admitted in the course of the twelve months. A review of the monthly admissions during four years, taken together, would go to prove that June and July are the months most favourable to the development of insanity. The admissions stand thus (for four years):—June, 86; July, 77; December, 66; April, 65; March and November, 62 each; May, 61; September, 59; August, 58; February and October, 57 each; January, 51. It is curious that the number of those admitted between twenty and thirty, between thirty and forty, and between forty and fifty was the same for each decennial period, viz., 52. None was admitted under fifteen; one man and one woman were admitted between seventy and eighty. The patients were classed as labouring under—

Melancholia,	-	-	-	75	males and 85	females
Mania,	-	-	-	53	„	50 „
Stupiditas,	-	-	-	4	„	1 „
Delirium tremens,	-	-	-	1	„	0 „
Dementia,	-	-	-	74	„	56 „
Idiotismus acquisitus,	-	-	-	9	„	7 „
Epilepsia,	-	-	-	11	„	3 „
Paralysis generalis,	-	-	-	9	„	1 „

Of these the author considers the first four forms to be primary, and in general curable, and the latter four to be secondary, and, as a rule, incurable; hence 133 males and 136 females would, on admission, have been looked upon as curable; while 103 males and 67 females would, on the contrary, have given no hope of recovery.

5 and 6. The Indices Scholarum, being little more than programmes of the lectures to be delivered in the University, do not require further notice at our hands. We may, however, mention that the arrangements of the institution appear to be very complete, and that the facilities for instruction afforded by the several museums, laboratories, libraries, collections of instruments, and by the Botanic Garden, would seem to be very great.

7. The object of Dr. Bidentkap's work is sufficiently set forth in its title, which we have quoted. It would be quite impossible here to follow the author through his able review of the opinions of those writers who have preceded him, and through the numerous cases and experiments upon the examination of which he has based his own conclusions. The result to which his laborious investigation has led him may probably be sufficiently gleaned from a perusal of the last paragraph of his book:—

“To the views above laid down, respecting the relation between the two kinds of chancre, the author has, it will be found, been led by observations and experiments recently instituted in the section of the Royal Hospital for Diseases of the Skin. But Professor W. Boeck had already, chiefly from the results of the continued inoculations during syphilization, arrived at similar conclusions, namely, that the virus which produces the non-infecting chancre is identical with that which gives rise to the infecting, only that in the first case it possesses greater intensity, and is consequently limited to the production of a merely local phenomenon.<sup>a</sup> This view, it will be seen, very closely resembles that above put forward.”

8. Dr. Höegh informs us that, between the 31st March, 1862, and the 4th May, 1863, he had under his care, in his district, containing a population of 8,700 individuals, 320 patients affected with diphtheria faucium, being 1 to every 27 of the population, or 3·6 per cent. Of these 320, 63 died, or 19·6 per cent., as follow:—

<sup>a</sup> W. Boeck, *Recherches sur la Syphilis*, p. 64.

	Years.		Treated.		Died.		Per cent.
	Under 1,	-	-	11	-	2	= 18
Between	1 and 5,	-	-	86	-	19	= 22
„	5 and 10,	-	-	90	-	20	= 22
„	10 and 15,	-	-	71	-	15	= 21
„	15 and 20,	-	-	31	-	6	= 19
„	20 and 30,	-	-	21	-	1	= 4.7
	Over 30,	-	-	10	-	0	= 0

As to sex there were of the 320—

166 males, of whom 35 died = 21 per cent.; and  
 154 females, „ 28 died = 18 „

These tables speak for themselves, and are sufficiently interesting. As sequelæ of this malignant epidemic of diphtheria, the author notes:—

	Cases.
Paralysis of the throat (difficulty of swallowing, nasal voice), in	84
„ of the muscles of the nape of the neck, in	- - 6
„ of the lower extremities, in	- - - 10
„ of the upper extremities, in	- - - 6
„ of nearly all the voluntary muscles, in	- - - 2
„ of the optic nerve, in	- - - 16
„ of the auditory nerve, in	- - - 12

Of other sequelæ there were:—

	Cases.
Long-continued weakness of all the vital functions (without paralysis), chiefly as a high degree of retardation of convalescence, in	- - - - - 10
Parotiditis, terminating in suppuration, with inflammation of the areolar tissue of the neck and formation of abscess, in	9
Furuncles, in	- - - - - 5
Exanthemata: erythema, vesicles, papulæ, and pustules, in	- 6

9. The Calendar of the University does not call for any special notice in this review.



*Étude Clinique sur Divers Symptômes Spinaux Observés dans la Fièvre Typhoïde.* Par le DR. E. FRITZ. Paris: A. Delahaye, 1864. Pp. 186.

THE monograph by Dr. Fritz which now lies before us for review, and which had the honour of being crowned with a gold medal, presents us with the results of his clinical researches "Respecting certain Spinal Symptoms occurring in Typhoid Fever." Although the title of this work would seem to limit those symptoms to follicular fever, it may be necessary, at the outset, to give a caution, which the author curiously enough has postponed to the close of his book, that though they more especially belong to typhoid fever, yet this ailment does not enjoy a monopoly of them, as they may be equally found in exanthematous typhus, in double pneumonia, and other acute diseases. His observations were made at Paris in the wards of Drs. Barthez and Tardieu, of the hospitals Sainte Eugénie and Lariboisière, and occupied the years 1863 and 1864. Dr. Fritz undertakes the discussion of the question, whether in the classic treatises which have been written upon typhoid fever the spinal cord has had its due share of notice, and arrives at the conclusion that it has not. "In the portioning-out the nervous phenomena of this disease between the different parts of the cerebro-spinal system and the sympathetic, the spinal cord has found itself almost completely disinherited." He asks us confidently to name a single treatise which devotes a special chapter to the spinal symptoms of typhoid fever.

It must be acknowledged that though we find frequent allusions to those symptoms in works on fever and general medicine, and especially in those of Eisenmann and Wunderlich, of Germany, and of Lombard and Fauconnet, the distinguished physicians of Geneva, yet as a whole, the rôle which the spinal cord plays is as yet imperfectly understood. In these countries, more particularly, we have scarcely thought it necessary to discriminate between cerebral and spinal phenomena. We anticipate, therefore, that our author has, in his borings into a new mine, lighted upon a lode which is likely to afford a respectable produce. His merit is, that he found certain nervous symptoms, described as unusual, *bizarres*, exceptional, and that he traced successfully the connexion which attaches them to the general history of enteric fever. He discards the ancient axiom "*rara non sunt artis*," as applying more to symptoms viewed in an

isolated relation. If we take the general rule as the point of departure, and work synthetically downwards, we may fall into the error of regarding many natural deviations as monstrosities; whereas, if we work upwards, and carefully analyze apparent exceptions by the inductive method of Bacon and his followers, we shall take the best course to push the boundaries of knowledge some steps forward. Such is the course adopted by the author, and his "Table des Matières" consists of eight chapters, through which he perseveringly traces the so-named spinal symptoms in their manifold relations.

The first chapter of the book is dedicated to a historic description of the state of our knowledge upon this subject from the earliest records which we possess down to the period in which the writer took up the question. We think that some objections lie against the custom which prevails in medical books, of giving the bibliography of a disease *before* an account is given of the disease itself. The reader is likely to begin by taking ancient errors for truths which he is afterwards obliged to unlearn as errors. It is like conducting a stranger through the miry lanes and tortuous bye-ways of a city, before showing him its magnificent streets and architectural ornaments. We would, therefore, relegate the genealogical tree of the disease to the end of the book; or if that may seem a violation of the order of nature, we would admonish the reader generally to skip the historic chapter until he had read the subsequent ones. In a critical review, however, we may not find it convenient to adopt this course; but still the author's historic sketch is scarcely intelligible unless we keep steadily in view the conclusion at which he wishes us to arrive.\* By carefully comparing different parts of his book we think that it may be expressed in the following proposition:—

*That typhoid fever with spinal symptoms is a disease totally different from cerebro-spinal meningitis, and that as yet we do not possess sufficiently satisfactory evidence that the latter disease can even complicate the former.*

There are two cases given by Piorry (pages 131 and 137), which appear to confirm the possibility of the two diseases co-existing; also a case of Drs. Lombard and Fauconnet, entitled "Typhoid Fever Complicated with Symptoms which appear to depend upon an Acute Inflammation of the Spinal Marrow, or at least of the Membranes enveloping the Spinal Cord" (page 132); but the testimony of these cases the author endeavours to controvert.

One of the earliest authorities upon spinal symptoms in fever

quoted in the sketch is Finke, who described an epidemic of bilious fever which occurred in 1776–1780, and particularly noticed the existence “of pains resembling rheumatism in the limbs and back, together with a prickling sensation in the muscles.” We could cap his quotation with one of earlier date from one of our own authors, as follows:—“A pain in the small of the back, except in cases of small-pox and measles, is a general attendant on putrid fever, and may be regarded as a pathognomonical symptom” (Fordyce on Fever, 1774). It is a curious coincidence that Lombard and Fauconnet put forward the very argument of Fordyce, that the existence of pain of the back is a valuable means of diagnosis in doubtful cases of typhoid fever.

Coming nearer our own time, the first author, according to Dr. Fritz, who started the question of relationship between typhoid fever and spinal meningitis, was Dr. Grossheim, a German physician, in 1835. He dissected the spine in twenty-one cases, and arrived at the singular conclusion that abdominal typhus was *always* complicated with inflammation of the spinal cord, a conclusion justly pronounced by Grisolle as one which “n’a aucune espèce de fondement.” The next attempt to explain those dorsal pains was to ascribe to them a rheumatic origin; and he comments on cases by Forget of Strasbourg, in 1841, and by Bazin, in 1834, of supposed rheumatic complications of typhoid fever. He examines Bazin’s so-called arthritic form of typhoid, and discards it because the plaques of Peyer showed no signs of ulceration. He charges Forget with an error of diagnosis, that his rheumatismal variety is a misnomer, and that all the strange symptoms dominant in the case, such as rachialgia, general cutaneous hyperesthesia, &c., and which are wholly inconsistent with ordinary rheumatic pains, receive a perfect explanation upon the spinal theory. But by far the most important observations upon this subject belong to MM. Lombard and Fauconnet in 1843. In an epidemic at Geneva in 1840, these physicians noticed in some cases cervical pain radiating to the arms; also pain in the spine and lower extremities, and on consideration did not hesitate to refer those symptoms to a morbid condition of the spinal cord. “Since our attention was awakened on this subject,” they observe, “we have questioned most of our patients, and have been truly astonished at the frequency of spinal symptoms of different degrees of intensity, and at different epochs of the malady.” They mark particularly the following sets of symptoms, which we give in their own words:—

“1°. *La Céphalalgie Occipitale et la douleur Cervicale.*—Nous les avons observées assez souvent, pour pouvoir les considérer comme devant entrer dans la description générale de la fièvre typhoïde 2°. La douleur occupant les dernières vertèbres cervicales, et les premières dorsales s'accompagnant dans certains cas d'une grande gêne de la respiration; de douleurs suraiguës dans les flancs, à l'épigastre et dans les parties latérales du thorax; d'engourdissement de la mâchoire et des bras. 3°. La douleur de la région dorso-lombaire de la moëlle épinière, à laquelle s'associaient des douleurs dans divers points de l'abdomen, quelquefois à l'épigastre, à la vessie, avec dysurie, incontinence ou rétention d'urine; enfin surtout la paralysie à divers degrés des extrémités inférieures.”—P. 17.

We are reluctantly compelled to refer our readers for the continuation of the history of these symptoms to the book itself, and now proceed to give, very briefly, Dr. Fritz's “*Exposé Analytique des Symptômes*,” in his second chapter, which occupies more than half the work.

The spinal symptoms are classed by him under two divisions; the first appertaining to the spinal cord in its entirety; the second having a distinct and special reference to the bulb.

The spinal symptoms, *properly so called*, comprise morbid disturbances, first, of the general sensibility of the cord, and secondly, of its motor functions. The general sensibility may be disturbed in three degrees; it may be abnormally increased, perverted, or diminished. As the author depends his case upon the first of those states, as that which carries with it “*le cachét le plus évident de son origine spinale*,” we shall confine our remarks chiefly to that condition.

The hyperesthesia of typhoid fever is composed, according to the author, of two elements, which habitually are combined in variable proportions; but one of them, at least, can exist independently of the other. Exalted sensibility may have its seat in the nerves of the external tegument, or in those which supply the muscles. A third form consists in an abnormal tenderness of the spinal apophyses, which, however, may be referred to the general hyperesthesia of the skin.

*Cutaneous Hyperesthesia.*—What are the distinguishing characteristics of this increased sensibility of the skin? As a general rule, which as yet holds good, it follows “*une marche régulièrement*



ascendante," and whenever a zone of the body becomes affected, it is to be found invariably in the parts situated beneath. As regards the parts engaged, the inferior extremities are most frequently affected. The next in the order of occurrence is the abdomen; in this it is usually diffused over the whole surface, but occasionally is concentrated in the iliac fossa; it is best perceived by pinching up lightly a fold of skin and pressing on it with the finger. After these in frequency come the thorax and back, then the superior extremities and the neck; but the increased sensibility always stops below the border of the lower jaw. The author gives cases intended to bear out these phenomena. We may remark that cutaneous hyperesthesia had been noticed by other observers before Dr. Fritz. We have before us a treatise on fever by Dr. Southwood Smith, published in 1830, from which we take the following extract:—

"It is not very common, but there sometimes takes place an extreme degree of tenderness over the entire surface of the body. The sensibility is so much increased that the patient cannot bear without pain the slightest pressure. Several cases have occurred in which the entire skin was as tender to the touch as the abdomen in some of the abdominal cases. Whenever this preternatural sensibility occurs, it is always in connexion with an exceedingly severe form of the disease" (page 173).

The same fact has been noticed by Dr. Graves as accompanying the epidemic of exanthematous typhus which occurred in 1834. (Clin. Lec., Vol. I., p. 231). Dr. Fritz accompanies the quotation, which he makes from the French translation by M. Jaccoud, which has already reached a second edition, with the observation—"It would be difficult to exaggerate the importance of the fact herein signified, *for it is Graves who speaks.*"

*Spinal Hyperesthesia.*—This form is more frequent but less regular than the cutaneous, and may exist in any part of the spinal column from the atlas to the sacrum. It is seldom met with throughout the whole extent; on the other hand Dr. Fritz has seen but one case wherein it was limited to a solitary spinal apophysis. It is carefully to be distinguished from rachialgia, with which it is often associated. In relative frequency of occurrence may be ranked the cervical vertebræ alone, the last cervical and first dorsal, and the dorsal. The test of its cutaneous character is this, that superficial pressure causes more pain than pressure firmly applied.

He found it to exist in various degrees in half his cases, and in more than half the cases of women and children. We must, however, not be contented with a verbal inquiry about it, as patients mostly reply to us in the negative; we must question the spine instead of the patient, and that in each successive vertebra.

After dwelling lightly upon muscular hyperesthesia, he discusses the various pains which patients refer to the back, all of which he includes under the general title of *rachialgia*. The subject is too extensive for us to enter on, especially as he connects it with radiations of pain to different parts of the body—with pains in the lower limbs, with pains in the chest, and with pains *en ceinture*. His description of perverted sensations is very brief, not occupying more than a dozen lines. He remarks on diminished and abolished sensation, of which he gives a curious case wherein a zone of natural sensibility interposed between two regions in which analgesia existed. We must also pass over the section upon the disturbance of the motor functions of the cord, though of much interest.

The most important part of the book in a practical application is that which describes those spinal symptoms whose origin is to be sought for in the medulla oblongata. They constitute a terrible array, a dark grouping in the general tableau of the disease. Among them are included sudden dyspnea without any thoracic affection, spasmodic cough, loss of the power of swallowing, speechlessness, spasms of the larynx, &c., &c. We would suggest to the author an expansion of this chapter in his second edition.

We pass from the chapters on the diagnosis, the etiology, and the prognosis of these spinal symptoms to the chapter on treatment, which is, in our opinion, the least satisfactory in the book. We do not attribute this to the fault of the author, but to the difficulty of the subject, namely, topical treatment in a general disease. In cases of severe pain of the back, which frictions with chloroform liniments are unable to relieve, he approves of cupping in preference to leeches. Bleeding he wholly discountenances. Blisters along the spine are more suited to the advanced stages, especially when loss of muscular power begins to be felt. Ice and all cold applications are particularly to be avoided in cases of grave spinal symptoms, such as spasm of the pharynx, &c., indicating disturbance of the bulb. For internal treatment he advises the use of purgatives; and thinks belladonna may be useful, while opium is dangerous. He makes no allusion to the use of mercury. As

according to his views typhoid is not complicated with spinal meningitis or myelitis, he declines entering into considerations upon the treatment of such an eventuality. He sums up with a useful caution respecting spinal cases, that the cord long retains a morbid sensibility which protracts convalescence. A good *résumé* of the chief subjects discussed closes the work.

We rise from the perusal of this interesting book with a high opinion of its author as a man of originality of mind. He has not wholly satisfied us with his views respecting the impossibility of typhoid fever co-existing with positive inflammation of the membranes of the brain or cord, and terminating in the effusion of plastic lymph. We readily admit the almost universal absence of pus or exudation corpuscles in the brain or spine in typhoid fever, even in those cases where the spinal symptoms have reached "*la violence la plus singulière*," but the curious epidemics of cerebro-spinal meningitis, such as have been described by Dr. Mayne in this periodical in 1846, and by continental writers, require the matter to be further investigated. It is no little merit in this age, when trite facts are put forward as novelties, to have a class of symptoms brought out in their proper place, which were either imperfectly known or kept heretofore in the background, besides cataloguing them under their proper headings. We trust that his work is but the pioneer of many future discoveries in the same wide field.

*The Optical Defects of the Eye, and their Consequences, Asthenopia and Strabismus.* By JOHN ZACHARIAH LAURENCE, F.R.C.S., M.B., Lond.; Surgeon to the Ophthalmic Hospital, Southwark, &c., &c. 8vo, pp. 112. London: Hardwicke. 1865.

*Ueber die Reihenfolge der Brillen-Brennweiten.* Von Dr. A. BUROW, Sen. 8vo, s. 20. Berlin: Peters. 1864.

*On the Serial Order of the Focal Lengths of Spectacles.* By DR. A. BUROW, Sen.

THE great work of Professor Donders, recently published in an English dress by the New Sydenham Society, appears, at first sight, to render other treatises upon the same subject almost superfluous. To the busy practitioner, however, we feel that the study



of such a volume is almost impossible, and we have, therefore, made a very full analytical review of it, which we complete in our present issue; and Mr. Laurence, influenced by much the same feelings, has put together some of the chief facts connected with this branch of ophthalmology, and has formed from them a treatise which, not only contains a great deal of practical information in an accessible form, and will enable those who peruse it to diagnose and relieve the ordinary cases of impaired vision from optical defects, but which is also calculated to lead the student to desire a further acquaintance with the important subject of which it treats. Mr. Laurence has, in some sense, a claim to occupy the position which he has thus taken, since he was the first to make known in England, by papers published in the *Medical Times and Gazette* for 1860, the nature of the doctrines that Professor Donders was then teaching, and of the researches in which he was engaged.

The treatise opens with a chapter on "Optical Considerations," which is chiefly devoted to a brief account of light, and of refraction and the formation of images by convex lenses. In this there is, of course, no room for anything new; but the author has accomplished his object with great clearness, simplicity, and brevity; and, especially in setting forth the nature of the relation between conjugate foci, with singular felicity of illustration. Mr. Laurence states in his Preface that for the last four or five years he has been daily engaged in the practical application of the principles that he explains, and his book thoroughly bears out the statement. It possesses the kind of clearness peculiar to a writer who has a working knowledge of his subject; and this clearness is the more remarkable since it owes little or nothing to the literary style of the descriptions of which it is a characteristic.

The second chapter deals with physiological optics—so far as it bears directly upon the main subject of the volume—that is to say, with the nature and mechanism of accommodation and of binocular vision. Here again Mr. Laurence is plain and intelligible, and has written just enough to meet the wants of a reader entirely unacquainted with the subject. We are disposed, however, to take exception to his account of the office of the iris in accommodation. His theory is that "the iris is an auxiliary organ for distinct vision—that the contraction of the pupil is intended as a corrective supplement to our accommodation, properly so called, by diminishing any slight circles of diffusion in the retinal image that might



possibly arise from inaccurate contraction of the ciliary muscle." It would, we conceive, be more correct to describe the office of the iris by saying that the contraction of the pupil, by cutting off the external and most divergent rays of a pencil, renders a smaller *amount* of accommodation effort necessary; and that it is an auxiliary destined to spare the labour of the ciliary muscle in degree, rather than to prevent confusion from its being imperfect in kind. On the subject of binocular vision Mr. Laurence contends, with reason, that the appearance of stereoscopic relief is due to a simple fusion of two perspectives, in opposition to the muscular sense hypothesis of Sir David Brewster. Like most other writers, Mr. Laurence speaks of the power of recognizing plane surfaces. His words imply the idea that a human infant, from the presence of certain appearances, finds out that certain things are solids, and have three dimensions. It is more probably true, from the vast preponderance of solids in the world, that the infant learns the occasional absence of the third dimension long after he is aware of its ordinary presence, and acquires the idea of a superficies at a time when the idea of a solid has already become perfectly familiar to him.

The third chapter, which is headed "Pathological Optics," contains only a definition of myopia and hypermetropia, and is followed by a fourth and fifth, which deal with these two conditions in their order. The methods of testing the state of the refraction, and of selecting the necessary spectacles, are clearly and concisely laid down. At p. 39, Mr. Laurence observes—"In testing each eye separately it is advisable for the surgeon simply to hold his hand in front of the eye not under examination; any compression of the eyeball by the patient's forcible closure of the eyelids may impede the free play of the eyeballs, and thus influence the refractive condition of the eye under examination." It is a better plan, we think, to add a small metal disk to the test case, and to fit this into the spectacle frame on the side corresponding to the eye not under examination.

The sixth chapter treats of astigmatism, and, from the comparative novelty of the subject, will probably be read with much attention. Mr. Laurence very clearly explains the nature of the affection, and gives some illustrative cases. His instructions for determining the kind and degree of astigmatism, and the character and strength of the glass required for its correction, are less explicit; but this, although technically an omission, is not one that need be regretted.

In truth, the examination of a case of astigmatism presents difficulties that can only be overcome by much experience. The surgeon is entirely dependent upon the statements made by the patient, who, on his part, is often inaccurate and confused. From this cause, as well as from varying states of the accommodation, different results are frequently obtained from successive trials; and, in order to explain and harmonize these differences, and to correct and allow for errors, a very complete and ready knowledge of all the factors of vision is absolutely required. For determining the state of the refraction in each meridian, as well as for ordinary cases of ametropia, Mr. Laurence recommends a simple and cheap optometer, made from a shoemaker's measure. This will do very well for a rough approximation, to be afterwards corrected by trials with lenses; but its graduation is not sufficiently fine to afford accurate results. By using a well-made instrument, in which the distance of the patient's cornea from the lens is fixed, and in which the distance of the test type from the optical centre of the lens can be read off by a Vernier scale to hundredths of an inch, spectacles may be ordered, for an intelligent patient, after one examination, with very tolerable certainty. Dr. Burow has pointed out that a star of thirty-six rays, placed in such an optometer, affords a ready means of determining astigmatism; and Mr. Laurence describes a method very recently introduced by Javal, who asserts that, in order to ascertain the most useful glass, both eyes should be tested together in the act of binocular vision. Nachet has made a binocular optometer for this purpose; but we have not as yet had any opportunity of determining its precise degree of utility.

Mr. Lawrence justly dwells upon the extreme importance of placing the axis of a cylindrical lens precisely in the right direction; and he figures and describes a useful little revolving instrument for measuring the angle which the axis should form with any chief meridian of the cornea. It will be found desirable, however, that the final adjustment of each lens should be made by the optician from absolute trial by the patient; and, in order to secure a good result, the caution given by Professor Donders about the perfect fitting of the frame should always be very carefully observed.

The seventh chapter is devoted to presbyopia. The discomfort arising from this condition has been much relieved by modern improvements in lighting; so that the presbyopic no longer discover their condition, as they did in Dr. Kitchener's time, by their tendency to bless continually the inventor of snuffers. Still, the

change which constitutes presbyopia being one that is common to all, and being, moreover, aggravated in the early stages of disease that imperatively demands treatment, this condition is, upon the whole, decidedly the most important of all those that require relief from spectacles. Mr. Laurence differs from Professor Donders in thinking that ten inches, rather than eight, should be held to be the distance of the near point necessary to constitute presbyopia; and he gives a short, but very clear, account of the nature of the defect, and of the means by which it may be remedied. The chapter concludes with a brief mention of the principle of Dr. Giraud Teulon's decentrated lenses; on the precise value of which Mr. Laurence declines, for the present, to express a positive opinion.

The three remaining chapters deal with paralysis of accommodation, asthenopia and strabismus. The first condition is especially likely to be overlooked; and Mr. Laurence has detailed some interesting illustrated cases, which will give the reader a very life-like conception of the disease. The chapter on asthenopia refers this condition—1, to optical defects of the eye; 2, to insufficiency of the internal recti; and, 3, to hyperesthesia of the retina; and gives instructions for the management of each. Under strabismus Mr. Laurence explains the manner in which this condition is brought about; describes an ingenious strabismometer of his own contrivance; and gives some statistics of the impairment of vision in strabismus, such as, we believe, have never before been published.

Upon the whole, we think that the purchasers of this unpretending treatise will find in it a guide that is at once concise, clear, and trustworthy; and that it contains sufficient information for the management of all ordinary cases of optical defect. It possesses the great merit of being practically original, even where it is not novel. Mr. Laurence has not compiled from German sources, but has worked at and assimilated his subject, and has then written down the principles and precepts on which his own experience has taught him to rely.

The pamphlet of Dr. Burow is intended to call attention to the great irregularity of the series of spectacle lenses contained in an ordinary test case; in which scarcely any two are separated by the same interval of difference. The difference between two lenses is of course to be expressed by the power of a third lens which, if added to the weaker, would make it equal to the stronger; and this power is found by dividing the sum of the focal lengths by their



difference. If we take  $F'$  and  $F''$  as the focal lengths of the two lenses, and  $F$  as the difference between them, then

$$\frac{1}{F} = \frac{1}{F'} - \frac{1}{F''}, \text{ and, therefore,}$$

$$F = \frac{F' \cdot F''}{F' - F''} \quad \text{Thus the}$$

difference between a lens of 20-inch and a lens of 30-inch focal length would be a lens of 60-inch; which, added to the latter, would make it equal to the former; for

$$\frac{30 \cdot 20}{30 - 20} = \frac{600}{10} = 60.$$

By applying the same rule to one of Paetz and Flohr's ordinary test cases, we find between Nos. 2 and  $2\frac{1}{2}$  a maximum difference equal to the power of a 10-inch lens; and between Nos. 36 and 40 a minimum difference equal to a 360-inch lens. In other words the maximum interval is thirty-six times as great as the minimum; and, moreover, with the exception of Nos. 20, 24, and 30, no two intervals are alike. From this defect Dr. Burow assigns two chief inconveniencies. In the first place some of the intervals are so much too large that certain eyes cannot be accurately fitted with spectacles at all; and, in the second, the cases in which the two eyes of the same person are of different powers, present peculiar difficulties.

For example, if a person requires for the right eye an 18 convex, and for the left eye a 10 convex, in order to read at ten inches, a time will come in which these glasses will require to be strengthened, on account of the increasing presbyopia of advancing life. The next number to 18 is 16, that is, an additional lens of 144; and the next to 10 is 9, or an additional lens of 90. The left eye will, therefore, receive a greater addition than the right, the near points of the two will no longer coincide, and binocular vision will be disturbed. Dr. Burow states, as the result of his optometric researches, that the refraction of the two eyes is somewhat different in nearly the half of mankind. Dr. Burow has found by experiment that, in complete paralysis of accommodation, there is still a certain small range of clear vision, due to the fact that the retina is insensitive to dispersion circles until they attain a certain magnitude. He finds that this range about equals a lens of 60 inches focal length; and he, therefore proposes such a lens as the fitting difference between successive lenses in the test case. To a series thus graduated he proposed to add a convex and concave of 120,



by which, in combination with others, the intervals of 60 can be reduced to 120, without multiplication of glasses. Test cases, fitted as described, are now supplied by Busch of Rathenow, and by Paetz and Flohr of Berlin; and, bearing in mind that the proposed improvement is only one of detail, it certainly possesses the merits of simplicity and convenience.

*On the Anomalies of Accommodation and Refraction of the Eye ; with a Preliminary Essay on Physiological Optics.* By F. C. DONDERS, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the Author's Manuscript by W. D. MOORE, M.D., Dub., &c., &c. London. 1864.

(Continued from Vol. xxxix., page 152.)

WE now proceed to briefly analyse the contents of the special part of this volume.

The fifth chapter treats of the emmetropic eye. We have already given (*vide* Vol. XXXIX., p. 140) the definition of this form of eye. It can be improved neither by convex nor concave glasses, and it can see accurately by means of its accommodation at relatively short distances. At one time it was thought that every eye was more or less myopic. But this is an error. In an absolutely mathematical sense no single eye is perhaps altogether emmetropic; but using this word in its ordinary sense, then, practically, the majority of the eyes of young persons is undoubtedly emmetropic. If it be asked, is this the most desirable condition, Professor Donders replies that, as far as regards himself, he would prefer a slight degree of myopia.

As in the translation of Donders' Memoir on Strabismus (*vide* Journal, Vol. XXXVII., 1864) the particulars of the centre of motion and movements of the eye, and the angle between the axis of the cornea and the visual line, were given in some detail, we shall no further refer to them here than to mention that in figures 4, 5, and 6, the axis of the cornea is  $a, b$ ; and the visual line, or line drawn from the retinal image of the fovea centralis towards its object is  $e, d$ .

With the increase of years the eyes undergo a number of changes of different kinds. Some of these are at once recognizable: such

as the diminished lustre of the cornea and of the conjunctiva, the smaller pupil, the changes of colour and less transparency of the sclerotic and of the iris, the diminished depth of the anterior chamber of the eye, the arcus senilis, and others. Some are only found on anatomical examination, such as secondary changes of the retina; calcareous deposits in the posterior parts of the sclerotic; changes in the choroid; atrophy of the ciliary muscle; greater firmness and a yellower tint of the lens, &c. Even during life the comparative ophthalmoscopic examination of sound eyes at different periods shows that with the increase of years the perfect clearness and deep transparency, in virtue of which the fundus oculi of a child is seen with such incomparable clearness, are lost. With such changes different disturbances of function are combined—there is a diminution of the accuracy of vision, and there is a lessening of the range of accommodation. The cause of the diminution of the accuracy of vision depends, first, on there being less accurate images on the perceptive layer of the retina, and secondly, on the perception and conduction being more imperfect.

Presbyopia depends on a diminution of the range of accommodation, the far point remaining the same, but the nearest point of distant vision recedes yet more and more. This progressive removal of  $p$  (the near point) is a fact of universal experience. It is generally thought that this retrogression first makes its appearance about the fortieth year; but this is a mistake. Although it is generally about this time of life that the retrogression of  $p$  makes itself felt as a *disturbance* in the normal eye, yet, even in youth so early as previous to puberty,  $p$  moves considerably backwards. This change, it is well to recollect, affects all eyes equally—the myopic (provided it be healthy) as well as the hypermetropic and emmetropic eye.

If we enquire on what the change in the power of accommodation depends, the answer appears to be—On the lens. As one advances in life the lens gets firmer. In consequence of this the same action of the ciliary muscle can no longer produce the same change in the form of the lens. The slight diminution of refraction must also, it appears to Professor Donders, be sought for in the lens. Thomas Young remarked, and it has been more fully demonstrated by Senff, Listing, and others, that a lens with the laminated structure, and with its refractive power diminishing towards the periphery, has a shorter focal distance than a lens of similar form, but composed wholly of a substance of the refractive power proper to its nucleus, would have. Consequently if, with the advance of years, the

outer layers become more solid, a greater focal distance must be the result. Now, the existence of some such increase of solidity is evident from the increased reflection, in advanced life, on the anterior and posterior surfaces of the lens—a reflection which is proportional to the difference in refractive power between the outer layers of the lens and the aqueous or vitreous humor. This is capable, too, of being proved by anatomical investigation. In favour of this view is the fact, that the diminution of refraction goes hand in hand with the diminution of the power of accommodation, thus pointing to a common origin.

One sometimes hears the word “*far-sighted*” used as a synonyme for presbyopia; but it is incorrect, for the presbyopic eye is not necessarily more far-sighted than an emmetropic eye. It is well, then, to restrict the definition of presbyopic “to that *condition in which, as the result of the increase of years, the range of accommodation is diminished, and the vision of near objects is interfered with*; not forgetting, as we have said before, that it is the normal quality of the normal eye in advanced age—no more an anomaly than are grey hairs or a wrinkling of the skin.

It is not possible to fix a definite time for the appearance of presbyopia; or, at least, in doing so the boundary must be artificial. But there is a practical use in having even an arbitrary selected limit; and Professor Donders considers the commencement of presbyopia to take place when the binocular nearest point  $p$ , lies at about eight inches from the eye, though this does not in general involve the use of spectacles.

There may be, as we have seen, presbyopia of the myopic and hypermetropic, as well as of the emmetropic eye. The hypermetropic eye must be called presbyopic as soon as, in the use of glasses, which neutralize the hypermetropia, the binocular nearest point lies farther from the eye than eight inches. As to the myopic eye, keeping to the definition of presbyopia, this must commence in it when the distance at the nearest point of distant vision amounts to more than eight inches. Hence it follows that it is only to the slight degrees of myopia that presbyopia can belong—that with  $M = \frac{1}{8}$  it is almost impossible. In slight degrees of myopia, presbyopia occurs much later than in the normal eye; and in this the advantage is by no means small. Not to require spectacles to see objects accurately until one’s sixtieth or even seventieth year, is a very great privilege, and one that belongs to a myopic person, whose myopia equals  $\frac{1}{10}$  to  $\frac{1}{14}$  when the eye is not threatened with

any special danger. With slighter degrees of myopia a good deal of this privilege is still retained. A knowledge of these facts is often of great importance; thus, should we inquire into the hereditary nature of myopia, its existence in the parents is often denied; yet almost in the same breath it is added, as a proof of their excellent sight, that up to their fiftieth year, or even longer, they still read and wrote in the evening without spectacles, and we at once know what inference is to be drawn.

One of the most characteristic marks of presbyopia is the correction, by means of positive glasses, in the commencement of the effort, of diminished accuracy of vision of near objects. As soon as, by diminution of accommodation, in ordinary work, the required accuracy of vision begins to fail, there is need of convex glasses. An opinion very often held, is that the longer one refrains from using glasses, so much the better for the eyes, but it is nothing else save the purest folly to wear out the eyes unnecessarily, instead of, by wearing proper glasses, restoring them to their normal condition. Strangely enough, others have fallen into an opposite error, and believe that by the early use of spectacles, the power of vision will be preserved, and so they use or recommend "conservative spectacles;" both errors are most objectionable—the latter appears closely connected with principles of self interest. The following circumstances may modify the degree of the glasses required:—*a. A range of accommodation not corresponding to the time of life.* Those who are very much occupied in reading, writing, or other work requiring close attention, usually believe, when their eyes have suffered, that they have demanded very much from them; but comparative observations show that much close work does not necessarily injure the normal eye, and that the range of accommodation, in such people, diminishes not at all more rapidly than it does in agriculturists, sailors, and others, who for the most part look to distant objects.

Many morbid conditions, however, cause a change in the range of accommodation, such as general debility, the result of exhausting diseases—premature old age, glaucoma, commencing cataract, and the like. It is most important to remember that in threatening glaucoma, where common prudence requires us to avoid all tension of the eyes, the use of spectacles to enable the patient to read or work at near objects should be avoided.

*b, Diminished Acuteness of Vision.*—Where acuteness of vision fails, we must, so far as is practicable, rather meet the difficulty



with larger objects than with larger images, by abnormally diminished distances of the objects. Where there is chronic opacity of the cornea, or uncomplicated incipient cataract, or senile amblyopia, we must rest content with recommending the use of larger type, or occupation with coarser work.

*c, The Nature of the Work to be Performed.*—The work may be such that even the young normal eye is insufficient. Minute drawing, engraving, watch-making, minute anatomical dissection, require the use of the magnifying glass. But apart from its minuteness, the nature of the work sometimes requires a definite distance; in the use of certain musical instruments, for example, the distance of distant vision has to be from one and a-half to two feet. It will be thus seen what an amount of care is requisite in selecting proper glasses for all these different sorts of cases. It is something of a comfort to know that, though essential, injuries to sight are often, with much exaggeration, predicted from the undue use of convex glasses, yet Professor Donders has never, in the course of his large experience, met with any.

Hypermetropia, of which we knew so little until the researches of Professor Donders, is the subject of the sixth chapter. We have already seen that it depends on shortness of the antero-posterior axis of the eye (*vide* Fig. 3, Vol. XXXIX., p, 141). It may be either acquired or original. Original hypermetropia may be again divided into *absolute*, *relative*, and *facultative*. The *absolute* exists when, even with the strongest convergence of the visual lines accommodation for parallel or diverging rays is not to be attained. In youth it is of rare occurrence. The *relative* exists when only by the strongest efforts this accommodation can be attained. The *facultative* exists when parallel visual lines can be brought to a focus on the retina. With the first, unassisted vision never can be acute; with the second, only monocular vision, and this exceptionally, can be so; with the last, on the contrary, binocular vision may also be acute.

The hypermetropically-formed eye is generally a small eye, indeed it may be said to be an imperfectly developed one. This structure, too, is *hereditary*, and is very probably congenital. The diagnosis of hypermetropia is very easy, by the aid of glasses; but Professor Donders points out peculiarities of structure, not merely in the flat anterior surface of the sclerotic, the shallow position of the iris, the relatively small pupils, the apparently diverging strabismus, but he sees in the very form of the face the existence of this anomaly. The margins of the socket are flatter, less round;

the whole face is flattened, with little relief; often, too, the nose is but slightly prominent, and the upper part of its bridge is so little marked that it can scarcely give support to ordinary spectacles. The eyelids are flat and broad; the eyes are far from one another. If this peculiar physiognomy is found in a young person, though the external appearance of the eyes betrays no sign of disease, we may reasonably infer the existence of hypermetropia. To the question, "Can you continue to work long?" we obtain almost always the answer, "No." The employment of glasses will not always prove the presence of hypermetropia; it may be latent, and we must induce mydriasis, thereby destroying the powers of accommodation, and so rendering the hypermetropia manifest.

Asthenopia is intimately connected with hypermetropia, and it is a subject on which it is most important to have correct notions. We will find eyes apparently normal, and yet in reading or other close work, especially in a gloomy place or by artificial light, the objects after a short time become confused, and a feeling of tension and fatigue comes on, in and especially above the eyes. The person so affected now often inadvertently closes his eyes, and rubs his hands over the forehead and eyelids; after a little rest he once more sees distinctly, but the same phenomena are again developed more rapidly than before; the longer the rest has lasted, the better is the result. At first this state of things was considered as a sort of amblyopia, but by degrees the cause was sought more and more in the organs of accommodation; but now there can be little doubt that hypermetropia is usually at the bottom of asthenopia. In former years the very worst consequences were predicted from asthenopia. It was considered to be the first stage of, or at least to be intimately connected with, amblyopia; and this latter, it was supposed, threatened the destruction of the eye, unless perfect rest was given to the eye. But though Professor Donders has seen hundreds of asthenopic patients, who from youth up to their thirtieth or fortieth year, and some to a more advanced period, had every day of their lives, without or with too weak spectacles, pushed the tension of accommodation to the uttermost, yet he never saw any diminution of the acuteness of vision arise from this cause. Many were the forms of treatment proposed to cure asthenopia. Some, as Mackenzie, considered it often incurable. "Many a poor man," says this distinguished author, "have I told to give up his sedentary trade and drive a horse and cart, while to those in better circumstances I have recommended emigration, telling them that

though they could not use their eyes advantageously in reading or writing, they might do well enough to follow the pastoral pursuits of an Australian colonist." Had hypermetropia been known as the cause of asthenopia, so very decided a step in the life of a man need, perhaps, not have been taken. But the prejudice against using sufficiently convex glasses was deeply rooted, for the same writer says:—"A child, the subject of asthenopia, engaged in learning his lessons, complains he cannot see, and repeats the complaint so frequently, especially by candlelight, that his father or grandfather at last says, 'Try my glasses.' *The child now sees perfectly*, and night after night the loan of the glasses is required before his task is finished." But instead of recommending the use of these glasses, he adds:—"It would have been better had glasses been selected at the longest focus that would have enabled the child to read, or *better still* that he had been put to bed, and the lesson left until daylight." Now asthenopia need no longer be an inconvenience to any one, for the discovery before alluded to pointed out the way to obviate it. This subject of hypermetropia, and its relations to asthenopia, is one of the most important in this very able work. Would space permit, we would gladly condense this whole chapter; but the connexion of hypermetropia with converging strabismus has already been fully discussed in our pages (Vol. XXXVII., p. 480), and we will only now subjoin the following most interesting supplementary cases. These illustrative cases are well worthy of a careful study:—

"We learn to distinguish, at first sight, an ordinary case of asthenopia, the result of H.

"II. Miss H., aged 19, is announced. She has a florid look, has clear eyes, without a trace of disease, blue iris, mobile pupil, not a very deep globe, flat margins to the socket, the visual axes appear to diverge. I suspect asthenopia. I make her read, and bring the book to 6": reading becomes difficult; at 5" it is impossible. There is either H. or diminished  $\frac{1}{A}$ . My eye falls on those about her, I see a brother with converging strabismus. This was decisive in favour of H. 'You cannot persevere with your work.' She answers: 'No.' 'On exertion you get a feeling of tension over the eyes, press upon the part with the hand, rub over the closed eyes, and then it passes off, but only for a short time?' 'Precisely,' is the answer. Confidence is gained. 'You have no pain in the eyes?' 'At a distance you see well?' 'Yes.' 'After a long rest you can continue your work better?' 'Yes, yes.'—With  $\frac{1}{18}$  she distin-



guishes well at a distance, and moving the glasses at the first moment not so well; with  $\frac{1}{16}$  not so acutely as with  $\frac{1}{18}$ , with  $\frac{1}{24}$  not more acutely; between the two eyes there is little difference. Ophthalmoscopically all is well. I learn further, that for some years the inconvenience felt in working has been always increasing; that formerly when weakened by fever, she could for a time neither read nor sew; that she once tried a pair of spectacles, but was strongly cautioned against wearing them, &c., &c. She gets spectacles of  $\frac{1}{16}$  to work with, with a recommendation now and then to pause for a little, and at first not to do much in the evening. At the end of a week she has forgotten her ailment. She now works too with less trouble occasionally for a short time without spectacles, which I advise her to do, though with the recommendation to return to the use of the spectacles on the least trace, or rather before the occurrence of fatigue.

“Asthenopics have sometimes a sad past, and live in a gloomy future.

“III. The Rev. G. D., aged 52, looks dejected. ‘My good Professor,’ he says, ‘I come to you, for I feel that I am getting blind!’ For the last twenty years he has thought that within a year he should be blind; and, singularly enough, although he still sees, he continues to look upon every year as the last! Such is the man! His life has been a struggle with his eyes. Even as a child he read with difficulty. When a student, the least exertion fatigued him, and he was compelled to learn more by hearing than by his own study. As a preacher, he has been obliged to write his sermons in a rather large hand, and still to get them off by heart. And, what was the worst part of it, he never read nor worked without the idea that he was thus hastening his blindness—interfering with the concentration of his mind upon any definite object. The same fear of blindness had restrained him from a matrimonial alliance with which he believed his happiness for life to be connected. He trusted in art. He had faith in a person he consulted in Germany; and if the optician had sometimes given him spectacles which had brought him relief, these were mercilessly taken from him again by the oculist on the first consultation, as a treacherous instrument which must, in the end, inflict upon him the total loss of his sight. At last he had, in his fortieth year, got convex glasses of  $\frac{1}{40}$ , and he now uses  $\frac{1}{20}$ . ‘Do you see with these spectacles at a distance?’ was my first inquiry. ‘Something better,’ he replied, ‘but still very imperfectly.’ I tried  $\frac{1}{10}$ : ‘Much better,’ was his verdict;—subsequently I gave him  $\frac{1}{8}$ : ‘Still better.’ In



a word there was  $H = \frac{1}{7}$  with  $S = \frac{17}{20}$ , and, with his slight range of accommodation, he needed glasses of  $\frac{1}{5.5}$ , in order to make reading at the distance of a foot easy. He got  $\frac{1}{7}$  to wear. The man was grateful as a child. He left me as one saved from destruction.—Such victims of the prejudice against the use of convex glasses are not uncommon.

“Where  $H$  exists, paralysis of accommodation may give rise to disquieting symptoms.

“IV. E. K., a boy of ten years, son of Dr. K., remarks in the morning that he is not in a condition to read. His father sees that the pupils are rather large and are immovable. Paralysis of accommodation occurs to him; but at a distance also, the boy cannot properly distinguish objects: ‘there must consequently be a lurking affection of the optic nerve or of the brain.’ He brings his son to me. I establish the fact of paralysis of the sphincter of the pupil in both eyes. Neither on convergence, nor on the incidence of strong light, does contraction of the pupil arise: accommodative and reflex movements are both absent. The inference that there is paralysis of accommodation is thus justified. Why cannot the boy see even at a distance? A glance with the ophthalmoscope clears all up: it appears I must accommodate about  $\frac{1}{12}$  in order to see in the uninverted image the fundus oculi; and, as I am emmetropic, our boy has therefore  $H$  of about  $\frac{1}{12}$ . With  $\frac{1}{12}$  he then saw admirably at a distance; with  $\frac{1}{6}$  he read at the distance of a foot. All fear of an affection of the system of the optic nerve was gone. In speaking of the anomalies of accommodation, I shall revert to such cases. Here it may suffice to observe that within four weeks the paralysis had given way, the  $H$  had again gradually become for the most part latent, and that in what the boy had to do or to read, he now no longer complained even of fatigue. In a few years asthenopia may be expected, and the use of the convex spectacles while working will then be indicated.”

The subject of aphakia concludes this chapter. The absence of the lens in the dioptric system of the eye is in many respects an important condition. It is therefore strange that it had not even received a name until Professor Donders called this condition by the name of aphakia. It occurs either as the result of operation for cataract, or of a wound, and sometimes spontaneously. From the discussion of this subject, we learn that if the eye was myopic before the development of cataract, weaker glasses are required

after the operation than if the eye had been normal. In aphakia a small pupil is of great advantage; therefore, when iridectomy is performed, in connexion with the extraction of the cataract, it ought to be performed superiorly. In this case the part which has suffered excision is almost completely covered by the upper eyelid.

As was evident from the facts mentioned above (Vol. XXXIX., p. 135), not the slightest trace of accommodation exists in aphakia. In old persons with imperfect acuteness of vision, one is apt to fancy that there is a certain amount of range in accommodation; but in young persons, in whom one might reasonably expect to find more accommodative power, it is evident on examination that it is entirely lost. Here, therefore, the accommodative power must be supplemented by the hand, by which the distance between the glass and the eye can be altered. The lens placed before the eye has been substituted for the crystalline. It can also take on itself the part of the accommodation. It cannot do this by altering its form to the lens in the eye, but it holds to the old theory, according to which the power of accommodation was made to depend on the displacement of the lens. Notwithstanding this artificial power of accommodation it is in general advisable in aphakia to use two pair of spectacles, one for distant, the other for close vision.

The Seventh Chapter treats of myopia.

We have already seen that in this condition of the eye the focus of the dioptric system lies in front of the retina (*vide* Fig. 2, p. 141, Vol. XXXIX), or that parallel rays derived from infinitely remote objects unite in the myopic eye in front of the retina. In general it is easily recognized—near objects are very distinctly seen; while at a distance vision is, on the contrary, diffuse. When letters of double magnitude are not seen at double the distance, we are in general safe in assuming the existence of myopia. We determine its degree by trying what the *weakest negative glass* is, with which vision is as acute as possible; and next to the examination with glasses is that with the ophthalmoscope. Position in society appears to have a great influence on the distribution of myopia, those in easy circumstances being much more afflicted with it. It is not, however, equally prevalent in all countries, but appears to be more specially proper to cultivated nations. It is very prevalent in Germany. The distribution of myopia indicates tension of the eyes for near objects, as its principal cause. But the explanation of this fact is not so very evident. Myopia depends on a prolongation of the visual axis, and this is not altered in accommodation for near objects

How, then, is this prolongation to be explained? First, by pressure of the muscles on the eyeball, in strong convergence of the visual axis; second, increased pressure of the fluids, resulting from accumulation of blood in the eyes in the stooping position; and third, congestive processes in the fundus oculi, which, leading to softening, gives rise to extension of the membranes. In connexion with these causes, the injurious effect of fine work is, by imperfect illumination, still more increased. A stooping position of the head in reading or writing is also injurious. To this is to be ascribed the fact, that in boarding schools where the pupils by bad light read small print in the evening, or write with pale ink, the foundation for myopia is oftentimes laid. While with watchmakers and other such artizans, although they sit all day long with a magnifying glass in one eye, we observe no development of myopia, undoubtedly because they fix their work only with one eye, and therefore converge but little; they, too, generally avoid a stooping position.

It is a very common and sad error, that the near-sighted eye is considered as particularly strong, and it is one that is believed in even by medical men, who surely ought to know better. But there need be no hesitation in asserting that the myopic eye is *not* a sound eye. In it there exists more than a simple anomaly of refraction—there exists a marked extension of the membranes, and if this has proceeded to a certain limit, it cannot remain stationary, and so progressive extension includes progressive myopia, which is a serious disease of the eye. In youth almost every myopia is progressive, and this is often continued with symptoms of irritation. This is the critical period for the myopic eye. If it does not increase too much it may become stationary, and may even decrease in advanced age. If it is developed in a high degree, it is difficult to set bounds to it, hence at this period it needs every care, and on this too much stress cannot be laid. If it continues progressive, the eye will soon, with troublesome symptoms, become less available; and it not unfrequently happens, that about fifty or sixty years of age, the power of vision, from separation of the retina, effusion of blood, or from atrophy and degeneration of the yellow spot, becomes irretrievably lost.

Myopia may be divided into *stationary*, *temporarily progressive*, and *permanently progressive*. High degrees of myopia never appear to be congenital, and it rarely arises after the fifteenth, and never after the twentieth year, in eyes that were previously normal. When this is supposed to be the case, it will be found that the primitive



slight degree of myopia was overlooked. It would appear that fundamentally emmetropic eyes seldom, fundamentally hypermetropic eyes perhaps never, become myopic; but that having once occurred a tendency to myopia is transmitted to posterity, and, under fresh exciting causes, is developed to its higher degrees. It is not necessary to detain the reader with all the ophthalmoscopic appearances of myopia. The principal changes are, atrophy of the choroid, or of the outside of the entrance of the optic nerve; and this, when highly developed, is combined with a change of form of the nerve surface, a straightened course of the vessels of the retina, incomplete diffused atrophy in other parts of the choroid, and morbid changes in the region of the yellow spot. All these are particularly alluded to and described in this volume. In reference to the question of the connexion of staphyloma posticum with inflammation, Professor Donders concludes, "that almost without exception the predisposition to staphyloma posticum exists at birth; that it is developed with symptoms of irritation, which in moderate degrees of staphyloma do not attain any great clinical importance; but that in the higher degrees an inflammatory state almost always occurs, at least at a somewhat more advanced time of life, as a result, and as a co-operative cause of the further development of the distention, and of the atrophy. The predisposing cause of the staphyloma would appear to be arrest of development."

In higher degrees of myopia there is a tendency to approach closer than is necessary to the objects, and this particularly at sedentary work, and so to assume a stooping position. With increasing myopia, too, the fancy increases of occupying oneself with small objects. Persons so affected prefer small type, accustom themselves to small hand-writing, and avoid long lines—by so doing of course they see more at once. Myopes accustom themselves, too, to approximate their eyelids so closely that it actually belongs to their physiognomy; those affected with it seldom fix the persons to whom they speak, because they but imperfectly distinguish their features. In general they have no correct idea of the impression which their person and their mind make upon one another; and according to their original disposition, a peculiar freeness and too great self-confidence—or else what is rarer, a more than ordinary bashfulness, are thence not unfrequently developed. In their bearing, and often also in their gait, a certain awkwardness is frequently manifested, by which some are recognized at a distance. Finally, much more of what passes in the world escapes them than they



themselves are aware of, and with respect to a number of things their knowledge is less correct, because they fill up what is deficient from their own imagination. Cardarus even says that myopes are particularly amorous, because that not observing defects, they look upon human beings as angels. The starry heavens they also see in extraordinary splendour.

Besides diminished acuteness of vision, myopes often suffer from the rapid supervention of fatigue, a feeling of tension in the eye, sometimes pain or pressure, and also from photopsia (false perception of light) and *muscæ volitantes*. These two last phenomena often constitute a permanent source of complaint, even when no particular state of irritation and no extraordinary diminution of the sharpness of vision exist. Ordinary *muscæ volitantes* are not attended with any essential inconvenience. Myopes are, however, troubled with them perhaps more than others, and with them they increase and become less transparent in advanced life. Photopsia generally accompanies the more acute amblyopic symptoms of myopia; sometimes it is constantly present, and will yield to no treatment. This phenomena must, it is feared, be regarded as a bad omen, and as a sign of diffused atrophy.

In the translation of Donders' Memoir,<sup>a</sup> the connexion of myopia with insufficiency of the internal muscles, and with diverging strabismus, has already been given, so that we need not again refer to this portion of the subject.

The cure of myopia belongs to the *pia vota*, but the task of the ophthalmic surgeon resolves itself into the following:—

1. To prevent the further development of the myopia, and the occurrence of secondary disturbances.
2. By means of suitable glasses to render the use of the myopic eye easier and safer.
3. To remove muscular asthenopia, by the use of glasses, or tenotomy.
4. To combat the secondary disturbances of the myopia.

We would that space permitted us to go into each of these subjects in detail; but we can at most only allude to the chief points, imploring the reader who may have to deal with this subject to read the *ipsissima verba* of the volume itself. I. Strong convergence must be avoided; the patient should look much at a distance; for near objects he should have spectacles, which bring the farthest

<sup>a</sup> L. c. p. 480.

point of distinct vision to a sufficient distance—say from sixteen to eighteen inches. Work should—every now and then—every half hour—be discontinued for a couple of minutes. A stooping position must be avoided with the greatest care; the height of the desk on which the book or paper rests should suit the height of the head, and the inclination should be as great as circumstances permit. In writing, the limits are, when the ink no longer flows from the pen; but even then a pencil may be used. Further, those who are highly myopic must be earnestly dissuaded from everything that gives rise to increased action of the heart, and to tendency of blood to the head, with a view both to limit the progress of the myopia and to prevent the occurrence of secondary affections.

II. The prescribing of spectacles for myopes is a matter of great importance; unsuitable glasses are here most dangerous. In hypermetropia we must be aware of giving too weak, but in myopia of giving too strong glasses. In some cases the myopia may be neutralized—such, for example, as when the glasses are used exclusively for distant vision, and when the myopia is slight in reference to the range of accommodation and the eye is otherwise healthy. But *many circumstances forbid us to completely neutralize the myopia*;—they are connected with. *a*, The degree of the myopia; when this equals  $\frac{1}{2}$  and upwards perfect neutralization is inconvenient, owing to the smallness of the images formed. *b*, The range of accommodation; where the patient is past five-and-thirty and glasses have not been used from youth the tension required to see near objects with properly neutralizing glasses is too great to be maintained. *c*, Acuteness of vision; when this is diminished there is great danger in giving strong glasses. *d*, Age; the influence of age is, for the most part, comprised in the diminution of the range of accommodation and of the acuteness of vision. *e*, The nature of the work and the distance at which it is to be performed; when the myopia is properly neutralized in youth and if the sight be otherwise good, the eyes will be able to perform any ordinary work at ordinary distances; but, when this has not been done, the object will be to bring the farthest point to that precise distance at which acute vision is requisite. Thus, for example, for ladies, in the reading of music, the far point must be brought to within eighteen to twenty-four inches; so, also, for seeing manuscripts on the lecture table or in the pulpit.

The first incorrect estimate of magnitude, distance, and form is very quickly lost on wearing spectacles; and it is remarkable how

the eyes, being accustomed to look nearly through the axes of the glasses gradually limit their movements; even on the removal of the glasses this limitation continues, and the required movement is obtained by turning the head, thus giving a peculiar bearing to myopic people accustomed to wear glasses.

III. In high degrees of myopia there is often insufficiency of the internal recti; this may be developed in different degrees, from relative to absolute diverging strabismus; this first makes itself known by muscular asthenopia in binocular vision for near objects. It is often very difficult to know what to do in such cases. In non-myopes, suffering from insufficiency of the internal recti, systematic practice with prismatic glasses is of the greatest service; but here this must not be thought of. Von Gräfe even decides that in cases of excessive myopia it is better to submit to the tendency to divergence; the chief remedy of all is tenotomy of the external rectus; even where myopia is the cause of the affection it may sometimes yield excellent results. The condition, *sine qua non*, for tenotomy is, that under the attempt at single vision a sufficient divergence of the visual lines should appear to be possible; but for a full discussion of this subject we must refer our readers to Von Gräfe's paper in the *Archiv für Ophthalmologie* or Giraud-Teulon's work *Sur le Strabisme*.

IV. Therapeutic treatment. For myopia, as such, there is no therapeutic treatment—myopia consisting in an anomaly of form capable of no improvement. Hygienic measures will, undoubtedly, often check its further development, but it is not unfrequently complicated with symptoms of irritation and inflammation, and with other pathological deviations of different kinds which partly proceed from, partly promote its further development; and with respect to these it is the duty of the therapist to the best of his ability to interfere; with a few hints as to therapeutic treatment this chapter closes.

The eighth chapter treats of astigmatism; this is divided into *a*, irregular astigmatism, where there is an aberration which has reference to the rays refracted in one and the same meridian; *b*, regular astigmatism, where there is an aberration dependent on the focal length of the different meridians of the light refracting system. The cause of regular astigmatism is to be partly sought for in the cornea; and measurements have shown that the cornea in its several meridians has a different radius of curvature, whereas irregular astigmatism depends on the lens, so that in aphakia it is absent,



however the lens also modifies in a degree irregular astigmatism. So long as astigmatism does not essentially diminish the acuteness of vision it may be called normal; if it amounts to  $\frac{1}{40}$  or more it must be considered as abnormal.

The peculiar phenomena attending astigmatism are given in minute detail, but our space will not allow us to do more than thus allude to them; of the methods used for its diagnosis we find those of Professor Donders, of Young, Airy, and of Stokes, the well-known professor at Cambridge. General rules are laid down for the employment of cylindrical glasses. Astigmatism would appear to be either congenital or acquired, but it is in most cases congenital—one of the parents labouring under the same defect, but it often happens that different children, born of the same parents, exhibit this anomaly. In the majority of cases both eyes are affected; often, however, one is completely or almost completely free. When a high degree of ametropia exists on one side, asymmetry of the bones surrounding the orbit is a very common phenomenon. Professor Donders has met with astigmatism more frequently in men than in women, but this may be accidental. Although time of life can have no influence on astigmatism yet the disturbance of vision from this cause is less when the power of accommodation remains active. Of congenital regular astigmatism of the cornea we have myopic, and hypermetropic astigmatism, simple and compound of both, and mixed astigmatism; of acquired regular astigmatism we find that “depending on the cornea” and that “seated in the lens.” Irregular astigmatism may, as well as the regular, be divided into normal and abnormal—the latter depending upon irregularities of the cornea as well as upon those of the lens; to it may be referred conical cornea, spots on the cornea, and some of the acute affections of the cornea, with transparent ulcers—the former depending on the structure of the lens, and these, too, as suggested by Bowman, occurring sometimes independent of cataract and sometimes attending its earlier stages. This chapter on astigmatism is one of the most important in this volume, and we regret very much that want of space will not permit us to append several of the accounts of cases.

Chapter nine concludes the special part, and treats of the difference of refraction in the two eyes; the statement is often made that the right and left eye usually differ considerably; but this is a mistake; like the organs of animal life in general the eyes present a great symmetry between the right and left side.



The second, and by far the shortest portion of this work, is devoted to the consideration of the "accommodation." Although differing from the "anomalies of refraction," inasmuch as that the latter are anomalies of the form and the former disturbances in the functions of muscles, the connexion between the two is too obvious to be overlooked. Accommodation depends on muscular action; its anomalies are proper to muscles in general; paralysis and spasm; in connexion with the former the action of mydriatics, in connexion with the latter that of myotics is to be studied. These subjects are treated of in three chapters. The first is on the influence of the nerves upon accommodation and upon the movements of the iris; the second is on paralysis and debility of accommodation; this chapter contains an important section on the paresis of accommodation after diphtheria; the third is on spasm of accommodation, and treats fully of the subject of the Calabar bean; this chapter concludes this volume of which we have thus given a very brief analysis, one that may just point out to the reader the place where he will be sure of finding all that is known on the subject. We have preferred to make this notice more analytical than critical. In doing so we know that we have selected the more difficult task, for the best book printed is never free from many errors, the pointing out of which is never an arduous undertaking. To condense an author's statements, and yet not to misrepresent them, is a very different matter; our only hope is, that in trying to do so here we may not have altogether failed. A well-known ophthalmic surgeon has lately stated, that as well might Great Britain still claim the United States of America as part of her kingdom, as general surgery assert that ophthalmology is nothing but a small portion of her own domain.<sup>a</sup> Such works as this of Donders' will go far to prove this to be a correct statement. What general surgeon will care to master all the intricacies of this volume and when will general surgery itself claim the same rank among the exact sciences which ophthalmic surgery now does; but be this as it may, one thing is certain, that no lately published work has done so much to advance the progress of this branch of surgery as this one of Professor Donders, so ably translated for us by Dr. W. D. Moore.

<sup>a</sup> *Vide* Revue de l'Ophthalmologie Contemporaine Par le Docteur Anagnostakis. Annales d'Oculistique, Tome liii. Juin, 1865. P. 270.

## PART III.

### MEDICAL MISCELLANY.

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*Reports, Retrospects, and Scientific Intelligence.*

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#### PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.<sup>a</sup>

DR. CHURCHILL, President.

*Malignant Osteosarcoma of the Tibia.*—DR. HAMILTON exhibited a specimen of malignant tumour engaging the upper part of the tibia. The subject of the disease was a young woman, twenty-three years of age. Three years ago she became affected with a pain in the leg, below the knee, particularly when walking, and she came into hospital under the care of his colleague, Dr. Hutton. There was then a painful tumour in the upper part of the limb, but it was inconsiderable in size; it was blistered, and she got a good deal better, and left the hospital. A year after she first felt the pain, she was on a ladder, and her foot slipped, and came down flat on the ground. She experienced a good deal of pain in the seat of the disease, and from that moment the tumour rapidly increased in size. When the tumour was small the patient could walk about, but after the fall from the ladder the limb became perfectly powerless. When she came into the hospital the second time there was a very large tumour, which at first sight appeared as if it were an enlargement of the knee-joint. A little examination, however, proved that that joint was not affected. The movements of the articulation were but little interfered with, and the patella moved freely over the condyles of the femur. The disease occupied nearly the upper half of the leg, and formed a very large lobulated tumour. Some of the lobules were the size of a walnut, others as large as half an orange; they were also discoloured, presenting a livid, red appearance. The tumour felt soft and elastic; and this elasticity resembled so much the presence of fluid that one would be inclined to say, "There is a collection of fluid here that ought to be let out." However, this elastic, pseudo-fluctuating feel often arose in malignant structures in which there were no fluid contents. The patient suffered no pain; the tumour was tender to a

<sup>a</sup> These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

very slight degree, and without pulsation. A careful examination failed to detect the presence of glandular or visceral disease. The young woman, though not robust, looked healthy; the circulation was quiet, and there was reason to hope (if hope of a favourable issue could be entertained in a case of malignant disease) that amputation might be the means of saving life. It was evident, from the size and aspect of the tumour, that the integument would soon give way, when the chances of success from operation would be greatly diminished. The limb was, therefore, removed by the circular incision, in the lower third of the thigh. There was no disease in the femur where it was sawn across, but the quantity of medullary matter was very considerable, and the compact tissue remarkably thin. When the knee-joint was laid open the investing cartilage of the femur was found healthy in all respects; and the same might be said regarding that of the patella, with the exception of a small spot in its centre, where absorption had begun. The bone had been pushed upwards by the growth of the tumour. A section carried through the centre of the tumour, and through the shaft of the tibia, disclosed a well-marked encephaloid structure, together with a number of cysts. The latter occupied the most inferior part of the morbid growth; they were of various sizes, the dimensions of some being considerable: their walls were partly bony and partly membranous; they contained, for the most part, a dark fluid blood. The encephaloid portion of the tumour was of a brownish-yellow or tawny hue, and scattered throughout it were circumscribed masses of coagulated blood.—*January 7, 1865.*

*Leucocythemia, with Cirrhosis of Liver.*—DR. HAYDEN said, that the specimen before the Society was taken from the body of a gentleman who had been a veterinary surgeon in the army, and served in India for seven years, and who came under his care on the 18th of September, 1862. He returned from India in the early part of that year, invalided, and had undergone a course of mercury for secondary syphilis, in London, after his return. He was of exceedingly delicate frame, and was pale, nervous, and considerably emaciated. His appetite was capricious; his bowels were irregular, and his pulse was feeble. He only procured sleep by the use of opium. An enlargement of the spleen was the only organic disease Dr. Hayden was able to detect. He was under his treatment for nine months. He suffered during this period from neuralgic pains of a slight character in several parts of the body; from periostitis of the tibia and of the cranium; and from an attack of testitis. In June, 1863, he went to the sea-side, and remained there six months. During this time his condition was similar to that already detailed. The urine was normal, with the single exception that it contained a large quantity of lithate of ammonia. He passed the winter of 1863 in private lodgings in Dublin, and went to the sea-side again in the following

season, where he remained six months. He was attacked with cachectic ulcers in both legs, of a formidable character, which, probably, were the result of the former syphilis, and about this time œdema of the feet appeared. He was frequently unable to take any food whatever. In the summer of that year he took the opinion of an eminent practitioner in Dublin, who advised him to go to Baréges and drink the waters there. He did so, and returned at the end of six weeks greatly debilitated. His face and skin were exceedingly pallid. The ulcers had healed during his absence. His extreme emaciation was due mainly to the fact that while in Baréges he suffered from continual diarrhœa, due to the use of the waters. In September, 1864, he (Dr. Hayden) saw him again, when he found, in addition to anasarca of the lower extremities, which had been previously noticed, that there was effusion into the peritoneum. He became a private patient in the Mater Misericordiæ Hospital on the 23rd of last November. Whilst in hospital he suffered from a most excruciating pain in the abdomen, caused by flatulent distention of the large intestine. About this time Dr. Hayden examined the patient's blood, by taking a drop from the point of his finger, and he found, on examining it under the microscope, that the white corpuscles were greatly in excess. The red corpuscles were almost outnumbered by the white. This fact, taken in connexion with enlargement of the spleen, induced him to think that he had to deal with a case of leucocythemia. No suggestion was made by his colleagues, who saw the patient in consultation with him, which induced him to depart from that view of the case. The accumulation of fluid in the peritoneum increased rapidly; his debility became extreme; the bowels were generally constipated, and the dejecta of a white colour, and exceedingly fetid. There was no trace of jaundice. He died on the 21st of last December. On making a *post mortem* examination, the left pleural cavity was found distended with serum, and the lower part of the corresponding lung was compressed. The right lung was emphysematous. Of this there was no evidence during the progress of the case, and it was obviously an occurrence that took place at a very late period of his life. The right lung presented an anatomical feature of interest—namely, that it consisted of five lobes instead of three. The heart was small, scarcely larger than that of a child five years old. The right cavities were normal, but the left ventricle was greatly contracted in diameter, and its walls thickened. The abdomen was found to contain an immense quantity of fluid. The spleen was found greatly enlarged, as had been anticipated; but on examining the liver he discovered a condition for which he was not prepared, namely, that the organ was in an early stage of cirrhosis. This condition was quite distinct on the inferior surface of the right lobe; the upper surface did not show it so much, still it could be traced, and along the edge it was manifest. The gall bladder was distended with bile. The posterior



surface of the bladder was found to be connected with the anterior surface of the rectum by a band of fibrin, and attached to the posterior false ligaments of the bladder he found four bodies of a very remarkable character. They were yellowish in colour, smooth on the surface, and varied in size from a hazel nut to a pea. Each was attached by a very fine pedicle to the peritoneum. When removed from the body blood issued from the interior of these bodies. On examining them under the microscope, he found that the surface consisted of serous epithelium, and that the yellow, friable substance forming the cortex was composed of cells varying in diameter from  $\frac{1}{1332}$  of an inch to  $\frac{1}{1777}$  of an inch. They were distended with fatty molecules. There was no fibrinous structure whatever in these bodies. The medullary or central portion consisted of a plexus of fine capillary blood-vessels. He could not find anything described as resembling them, with the exception that Rokitski describes some bodies of this kind as of a strumous character. The structure of these bodies would lead to the conclusion that they were of that nature. It was possible that there was first congestion or vascular distention of particular parts of the peritoneum, that a deposit was formed round that as a centre, and that the epithelium was raised in that way, and the plexus of fine blood-vessels thus became encapsuled. He offered this merely as a hypothesis, in the absence of any more satisfactory explanation. The kidneys were in an early stage of fatty degeneration, and one spot was to be seen by the naked eye consisting of fat globules. They were of considerable size, and the right one was taken from the body without its supra-renal capsule, which was found attached to the inferior surface of the right lobe of the liver.

This might explain the occasionally reported absence of one of the supra-renal capsules, which, in these instances, probably remained attached to the liver, and escaped notice. This case was of interest in many respects. In the first place it was a case of cirrhosis of the liver in a man reported to be of exceedingly temperate and regular habits. Secondly, the fact that this cirrhosis of the liver was not detected or satisfactorily diagnosed, owing to the absence of jaundice or bile in the urine, and of hemorrhage from the stomach or bowels at any period. He was led to suspect that there might be disease of the liver, from two circumstances—one, the distention of the superficial veins of the abdomen and chest, and the other that the stools exhibited a want of bile, and the dejecta were exceedingly fetid.—*January 7th, 1865.*

*Cerebral Agnesia.*—DR. PEEBLES said, the case which he had the honour to bring under the notice of the society was one of cerebral agnesia. It had excited some interest at the other side of the Channel, where he believed a true diagnosis of it had not been made. He was happy to say that a distinguished member of the society—Professor

Law—was the first who stated its real nature. The subject was a girl aged nine years and ten months. Up to the age of eleven months, she was considered to be remarkably healthy and well-formed, and presented no appearance of paralysis. About this time a general practitioner prescribed low diet, with small doses of grey powder, for a feverish attack accompanying dentition. Soon after, for a slight cold, he directed tartar-emetic, and also two leeches to be placed on the dorsum of the foot. The low diet to be continued, and the leech-bites to be sponged with warm water to encourage bleeding. The attendants found it impossible to arrest the hemorrhage, and convulsions, followed by coma, were the result. Further advice was obtained, and then the head and spine were blistered. At this time the parents of the child, who had been from home, returned, and succeeded in getting some beef-tea, &c., into the stomach. Re-animation slowly returned, but it was found that all power of voluntary motion was absent. In the course of a year the left side recovered, but the right continued paralyzed. There was some difference in the thickness of the limbs, but none in the length. She now began to walk, dragging the right leg; and from want of power in the side met with several severe falls, the head generally coming to the ground with violence. On one occasion she fell out of bed head foremost; this was followed by epilepsy, which continued up to her death. At first the fits came on every ten minutes during the twenty-four hours. Afterwards they decreased in number, but increased in violence. The approach of a fit could be predicted by the development of mischievous propensities—a tendency to torment animals, or to strike people when they least expected it, and by considerable cunning. There was no scream before the fit. She always gave notice when it was coming on. The tongue was never bitten. There was a discharge of fetid perspiration, and seldom any dulness or drowsiness when the fit subsided. Her mental capacity was of the average; in some matters she was very intelligent, and had made some progress in her education.

She had been seen by various medical men, who attributed her ailment to either the loss of blood causing the development of tubercle in the brain, or to atrophy of the brain from the same cause. At the age of six years the left side again became paralyzed, as well as the right; but under tonic treatment the power over it returned. In last September Professor Law happened to be in the neighbourhood of where she was, and expressed a desire to see her. He was at once struck with a slight want of symmetry in the sides of the head, although measurement showed no difference, and pronounced it to be a case of congenital malformation of the left lobe of the cerebrum, similar to those which Professor Smith had brought before the society on several occasions. He (Dr. Peebles) first saw the child about two months ago, when she came under his care. Up to that time she had been able to walk three or four miles in the day

with the assistance of a servant to hold her hand. There was no difference in the thickness of the limbs, as she had grown tall and thin, but the heel of the right side was raised from the ground by the contraction of the ham-string muscles. The right arm was kept in the flexed position, with the hand hanging down; but by a strong effort of the will she was able to use it for various purposes. This, however, did not last long, the strength seemed to leave it very soon. There was a peculiar bulging of right side of the forehead, and the right cheek was drooped.

Although there was considerable power of voluntary motion in the right side, the galvanic stimulus did not appear to have any effect on the muscles, and wasted as they were, their contractive power in a fit was very great.

During the damp weather in November symptoms of inflammation of the membranes came on, quickly followed by those of effusion; but no pain was ever complained of, except a deep-seated one between the eyes.

She lingered for six weeks. Diarrhea set in, and bed-sores appeared wherever there was the least pressure, even over the left malar bone, where the cheek rested on the pillow.

As Professor Smith is our chief authority on this affection, and as the case differed in some respects from those which he has published, I requested him to make a *post mortem* examination. He (Dr. Smith) says:—"The right side of the forehead and of the head generally seemed more prominent and bulging than the left. On removing the calvarium, and making an opening through the dura mater, a large quantity of serum flowed out; and on removing the dura mater a layer of recently-formed coagulable lymph was seen covering nearly the entire of the left hemisphere of the brain. A similar material, mixed with serum, filled the sulci between the convolutions of the brain on the left side. The left hemisphere was much smaller than the right, more especially in front, the anterior lobe of the right side projecting at least half an inch in front of that of the left side. On making a vertical section of the left hemisphere a cavity or cyst was found in its substance, capable of holding a filbert. It was filled with serum, and lined by a dark brown membrane. The membranes of the brain adhered closely to that portion of the surface of the hemisphere that corresponded to the cyst. The latter was evidently the consequence of an original arrest of development, as was also the shortness of the left anterior lobe, and the generally small dimensions of the convolutions. The roof of the left orbit was much more prominent, internally, than that of the right, and the crista galli was strangely deformed. It was enlarged and curved in such a manner as to be placed to the left of the middle line, and nearly obliterate altogether the fissure for the passage of the nasal nerve."

This case differs from those which have been published by Professor Smith and others, in the absence of rigidity, and in the degree of



voluntary motion in the affected side—in the fact of the cyst or deficiency being in the substance of the brain, and not connected with the ventricle; and in the amount of mental development which was equal to the average of children of the same age. The peculiar appearance of the crista galli closely resembles the delineation in the plates published by Professor Schroeder Van der Kolk of his case of atrophy of the left hemisphere of the brain. The roof of the left orbit was also altered in the same way, but the resemblance goes no further. In his case the calvarium was thickened; in this case it was, if anything, thinner than natural. Measurement showed no difference in the sides of the skull; the space where the brain was deficient was filled with serum.

There was no appearance in the brain to account for paralysis on the left side. In the first attack the loss of power on that side was evidently the result of great prostration, as it was, most probably at the second attack, for at that time she was exhausted by diarrhea and hectic fever.—*January 14th, 1865.*

*Fibrinous Tumour.*—MR. COLLIS said the specimen which he had to exhibit was removed from the breast of a married female, aged twenty-seven. It was an example of what was described, many years ago, by Mr. Hey, as fungus hematodes. In Mr. Hey's description of it, however, two distinct classes of tumour were mixed up, and not sufficiently differentiated. There was the genuine encephaloid cancer in a state of fungation—a condition to which every tumour was liable; and there was, secondly, the fibrinous, or hemorrhagic tumour—a tumour different from encephaloid cancer, and extremely rare in the breast, although met with frequently in other situations. There was another peculiarity about this specimen in which it differed from the ordinary form of the disease met with elsewhere. Generally speaking, this variety of tumour was encysted—the hemorrhage takes place into the cyst—the clots become organized, and cell-growths take place in them; whereas, in this instance, so far as could be ascertained, there appeared to be no proper cyst, the tumour being diffused through the breast. The history of this case was briefly as follows:—On September 28th, 1864, he first saw this patient at the Coombe Hospital, where she was under the care of Dr. Kidd. She had then a large tumour of the right breast, which arose from a blow received in January, 1864. This caused a small kernel, which increased with tolerable regularity to the size of a small child's head. The tumour was globular in outline, and was much discoloured on the surface with numerous small varicosities, such as was to be seen in integument when distended over a tumour of any kind. It was remarkably heavy, and this was a special characteristic which distinguished it from genuine cancer. It could be moved about and lifted up from the pectoral muscle. Over much of the surface there was a



distinct fluctuation, but the deeper portions of the tumour had a very firm, doughy feel. There were no enlarged glands, nor any pain experienced except at the menstrual periods, in August and September, when the tumour grew with increased rapidity. The diagnosis which he made was that it belonged to the class of fibrinous tumours. The case of fungus hematodes of the tibia, lately exhibited by Mr. Hamilton, was the same disease as this; but in that instance the morbid growth was developed within cysts. The patient left the Coombe Hospital, and in December was admitted into the Meath Hospital, under the care of Mr. Collis. The centre of the tumour was now occupied by a large slough, portions of which came away from day to day. She complained, as before, of the great weight of the tumour, and of the unpleasant smell that proceeded from it. The sloughing continued, but without any diminution in the size of the tumour; it grew as rapidly as portions of it perished. There was no pain experienced except that attending the sloughing. There were no enlarged glands until the day before the operation, when the glands in the axilla became enlarged and inflamed, as they naturally would from the contiguity of so large a tumour in a state of high inflammation. He removed the tumour on the 20th Dec., by a circular incision, which in these large tumours Mr. Collis thought gave the best means of arranging the cicatrix in the most suitable direction. The wound rapidly healed, and was now all but well, the cicatrix being almost linear. He had already stated that this tumour belonged to one of two classes described by Hey as fungus hematodes. It was a remarkable fact that this eminent surgeon was able, even at that period, to distinguish the tumour in many instances as a non-cancerous one. His close observation of nature enabled him to make this distinction between it and the ordinary fungating cancer; but later writers had not been so careful, and hence the name of fungus hematodes has been applied indiscriminately to encephaloid cancer, and to that even when it was not fungating. This tumour was extremely heavy, much more so than an encephaloid tumour of the same bulk would be. Now, if they examined its component cells under the microscope, they would find that they were small, with sharp outlines, and a good example of a tolerably dense and firm lymph cell. They were not easily broken up by chemicals or by pressure, nor did they decompose and perish as rapidly as the cells of cancer. On examining the cancer cell under the microscope they found that it was a large, soft, weak, and delicate cell, and probably light—in other words, that there was the same amount of material in the larger cancer cell as in the smaller lymph cells; hence it was very intelligible that encephaloid cancers should be light as compared with these fibrinous tumours. Mr. Collis had termed this tumour, from its structure and its tendency to bleed, the hemorrhagic fibrinous tumour, and by these names it is now recognized by some other surgeons who have met with it.—*Jan. 14, 1865.*

*Tuberculosis*.—DR. HAUGHTON brought under the notice of the society specimens of extensive strumous disease removed from the body of a large baboon (*Macacus Nemistrinus*), who had lately died in the Zoological Gardens. It was a common opinion, and published in many books, that tubercular disease was the usual cause of death among monkeys in this country. He had found that tubercle was of very rare occurrence amongst those that died during the last six years in the gardens of this city. The present was the first case he had met with in which the cause of death was tuberculosis. The animals whose bodies he had examined had generally died of one or other of two causes—of scurvy, arising from the sameness of the diet to which they were subjected in confinement, or from pneumonia, caused by a sudden access of cold in the damp weather of this climate. The macacus before them took ill about four weeks ago, and became evidently worse from day to day, though various remedies were tried with him, and he died rather suddenly on Friday morning. On examining the body he found that most of the thoracic and abdominal viscera were the seat of tubercular disease; miliary tubercles occupied both lungs throughout their entire extent. The progress of the disease had been such as to show that it appeared latest in the lungs. It was incipient in the lungs, and two or three deposits of a similar nature existed on the surface of the heart. The liver consisted of five lobes, four of which were filled throughout their substance with hard tubercular masses, and tubercles were scattered throughout the substance of the spleen. The mesenteric glands were conglomerated into one large, hard, tubercular mass; yet there had been no diarrhea during the progress of the disease, and exceedingly little cough. Dr. Haughton said that this was the first case that came under his observation, after a large experience in the dissection of these animals, of death arising from tubercular disease. The knee-joints exhibited the anatomical characters of chronic rheumatic arthritis. The patella was displaced outwards, and the outer margin of the external condyle of the femur absorbed on each side. It was remarkable that on the dissection of the lion, which died of fever a year ago, they also found rheumatic arthritis in the shoulder-joint, and the condition of the diseased bone was identical in the two cases. It was almost certain that this disease originated in a state of confinement, for the bone was completely eroded, and showed none of that polished ivory surface which it would be likely to have if the animal were capable of exercising and in a state of nature.—*January 14, 1865.*

*Disease of the Aorta*.—DR. HEAD brought under the notice of the society a case of atheromatous and calcareous deposits in the aorta. The patient in this case was admitted into hospital in an almost dying state; his general appearance was that exhibited in an advanced stage of Bright's disease; he had a pallid countenance, feeble pulse, slow breathing, and

albuminous urine, with a slight degree of anasarca. On examining the urine, however, it was found to be normal in quantity and density; the albumen was small in amount, and there were no tube casts. The action of the heart was very feeble. There was dulness on percussion over a much larger surface than usual, and the impulse was very diffused, but there was no abnormal sound in the heart or aorta. This might be due to its feeble action. Upon examination, *post mortem*, it was found that the valves of the aorta were perfectly healthy; the atheromatous and bony deposit did not extend beyond the sulci at the back of the semi-lunar valves. The valves themselves were sound, and the aortic opening was not dilated, although the aorta itself was very much so. There was some degree of dilatation of the right ventricle, and very great hypertrophy of the walls of the left, which were more than three-quarters of an inch thick. Probably, if there had been a longer time to observe this case, and the action of the heart had been stronger, the usual sounds would have been recognized. There was extreme atrophy of the spleen; it was smaller than any spleen described, with the exception of one recorded by Andral. The structure was more dense than usual, and there could be traced fibrous bands through it, somewhat the same as in cases of cirrhosis of the liver. On the surface lymph was diffused, and there were small deposits of a cartilaginous hardness. The kidneys were hard and lobulated, and the left contained several cysts.—*Jan. 14, 1865.*

*Apoplexy.*—MR. ADAMS detailed the following case, and exhibited the morbid specimens:—

A coachman, aged sixty-three, was admitted into the Richmond Hospital, December 9th, 1864, under the care of Mr. Adams, complaining of great sickness of stomach and dizziness in his head, also of severe pain in the left eye; he had continual thirst and constipated bowels. He was of full habit, but has lost flesh very much lately, and has now a pallid appearance. About three months ago he began to lose his appetite and suffer from sickness of stomach; his eyes at this time, as he expressed it, became slightly clouded, and the left one, a few days ago, became very painful. He then applied to Mr. Adams who admitted him into the Richmond Hospital. He stated that some years ago he had suffered from pains in the loins, but these had been relieved by the use of flaxseed tea and emollient poultices. Up to the time of his admission he had been able to retain his place and attend to his horses. On admission there was no pain nor tenderness anywhere in the abdomen; no anasarca; the left eye was inflamed, and there was much subconjunctival serous effusion; the pupil was dilated, and the eye amaurotic; the tongue was foul; pulse 92, and intermittent; the urine was high coloured; density, 1013; it deposited on cooling a dense, flocculent, red material; it contained albumen in considerable quantity, and when microscopically



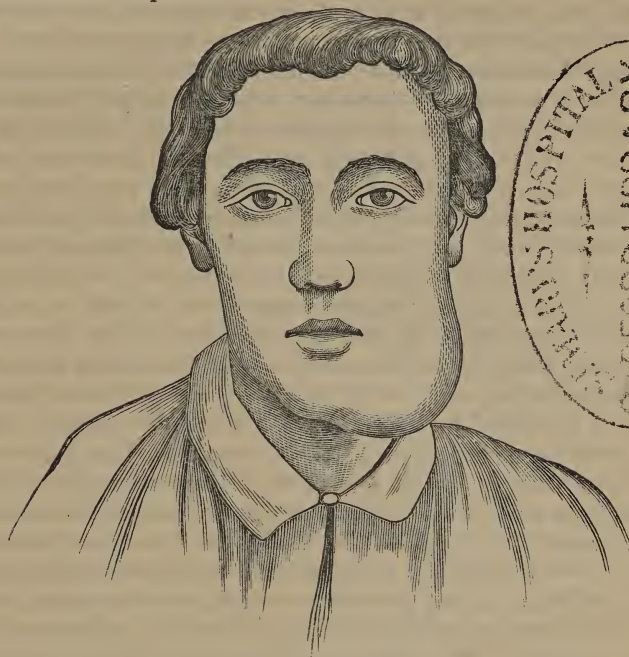
examined blood corpuscles and epithelial casts of the tubuli uriniferi were discovered. Under treatment the blood disappeared from the urine, and the eye became much less inflamed, and the effusion in a great measure disappeared, but the amaurotic condition remained and the albumen still was present in the urine. On the 8th of January he appeared better, but exceedingly weak; his pulse was 94 and intermittent, and he had lost flesh considerably since his admission. On the morning of January 11th, about four o'clock, he was found dead. One of the patients near him said that he observed him to snore louder than usual about three o'clock that morning. An examination of the body was made about twenty hours after death. The heart was large, and weighed 22 oz.; the surface was fatty, and there were some atheromatous deposits in the aorta; the muscular tissue of the heart gave way under the slightest pressure; all the valves were perfect; the liver was of its normal size, but the kidneys were enlarged, and weighed 7 oz. each. On section the surface did not present the usual difference in appearance between the cortical and tubular portions, the pyramids were interspersed with fat; the external surface was of a pale colour mottled with yellow, and both organs were enveloped by large masses of fat. A large quantity of effused blood was found at the base of the brain, and also a firm clot was seen occupying the position of the "circle of Willis." The arteries of the brain contained dark-coloured blood; their coats were somewhat thickened, and were of a yellow colour in various parts. A large clot of blood was found in the cerebellum, very near that part where the pons varolii and cerebellum join superiorly and laterally; here the upper portion of the crura and neighbouring part of the cerebellum had undergone some laceration of structure, into which parts an infiltration of blood had occurred. The source of the bleeding could not be traced to any particular vessel.—*January 14, 1865.*

*Cystic Disease of the Lower Jaw.*—DR. FLEMING said that the drawings which he begged to lay before the society, and the accompanying morbid specimens, illustrated the appearances present in a case of "Cystic Disease of the Lower Jaw," previous and subsequent to its removal. The subject of the disease was a young man, aged eighteen years, who had always enjoyed excellent health, and complained only of the deformity produced by a tumour on the left side of his jaw. On enquiry into the history of this tumour, it appeared that about eight months previous to his application at hospital he discovered a small immovable growth, about the size of a pea, on the external surface of the jaw, somewhat about the line of the second molar tooth. It was unaccompanied by pain or uneasiness, and, gradually increasing in size, at length formed a tumour which extended from the chin to beyond the angle of the jaw; the integuments were in every respect normal, and perfectly movable over the morbid



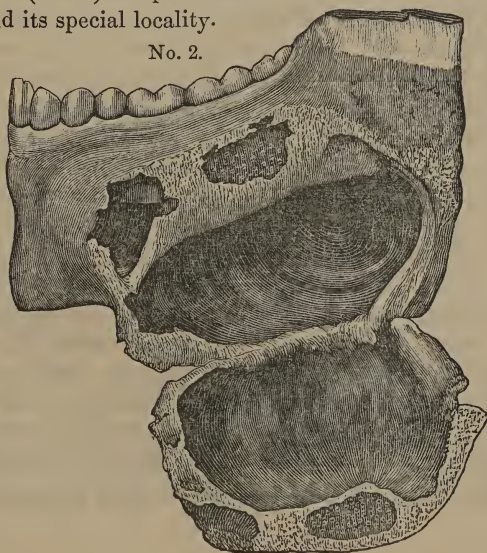
growth, which was obviously identified with the bone. On examining the inner and outer aspects of the jaw, the body of the latter was found to be considerably and widely expanded, especially towards the mouth and the side of the tongue, but the boy did not experience the slightest difficulty in mastication or deglutition, or in any of the ordinary movements of the tongue. It was, moreover, remarkable that the alveolar portion of the jaw was not materially implicated in the disease, and that the teeth contained in it, with the exception of the last molar, which was a little loose, were firmly fixed in their sockets. The crackling feel on pressing on different portions of the tumour, noticed by Dupuytren as characteristic of the osseous cysts of the jaw, was remarkably distinct in this case. The consideration as to the most advisable operative procedure was of some moment, namely, whether the tumour or cyst should be treated according to Dupuytren's suggestion, and a cure be thus attempted, or whether, on the other hand, the portion of the jaw involved in the disease should be wholly removed. On close investigation, however, it was quite evident that the body and base of the jaw were engaged in the tumour, their natural outline being wholly effaced, hence, that any attempt to effect a cure by laying open the cyst, or removing a portion of it, would have ultimately, in all probability, failed. The portion of the bone intervening between the symphysis and the lower half of the ascending ramus was, therefore, removed, and the society had now an opportunity of observing the pathological characters of the tumour. The altered shape of the angle and body of the jaw—the peculiar conformation of the cystic degeneration present—the varying consistence of the walls of the cyst, in some portions a mere osseous shell, and in others devoid of a particle of bony material, were well shown in a section of the tumour made by Professor Smith. The cyst was *unilocular*—contained a brownish coloured fluid, and was lined with a delicate and vascular membrane. With the view of contrasting the *multilocular* form of the disease, Dr. Fleming exhibited an admirable drawing of the latter in a case which was brought under the notice of the society by Dr. Hutton, during the last or previous session, and in which the removal of a similar portion of the lower jaw of a young woman was followed by complete success. Dr. Fleming remarked that it was desirable to take every advantage of studying the pathology of this disease, and thus to be enabled to ascertain its presence at the earliest period of its growth; hence, he thought that specimens illustrative of it should be brought under the notice of the society when ever an opportunity was afforded of so doing. Cases such as those operated upon by the late Mr. Cusack were now rarely, if ever, to be met with, and this was in a great measure attributable to the improved state of our knowledge respecting the pathology of the disease, its detection in its earlier stages, and its appropriate treatment.—*January 21, 1865.*

The accompanying woodcuts of Mr. Oldham are faithful delineations from Mr. Conolly's drawings. The first (No. 1) exhibits the deformity present before the operation. No. 1.



The second (No. 2) the peculiarities of the section of the tumour when removed, and its special locality.

No. 2.



*Melanoma of the Eye-ball.*—MR. WILSON said the specimen of disease of the globe of the eye which he now laid before the society had been removed from a female, aged sixty, a patient in St. Mark's Ophthalmic Hospital. The history of her case was as follows:—About eight years ago she was seized with very violent pain in the right eye-ball, which for several days continued unabated; the lids swelled to a considerable size, but there was no purulent discharge. After a fortnight, during which she was treated by leeches and purgatives, the swelling subsided and she got perfectly well, vision not having been affected. After the lapse of three months she got a similar attack, and during eight years subsequently she got repeated and violent attacks of the same description, after each of which, for four years from the commencement of the pain, the power of vision became diminished, and then was totally lost. Sometimes the pain was so intense, and the general constitutional disturbance so great, and of such long duration, that she was confined to her bed for months. The eye then ceased to be the source of much uneasiness until June, 1864, when she experienced violent pain in it, which extended over the side of the head, and was somewhat of a neuralgic character. There was no protrusion of the globe of the eye at this time. In July last she noticed, at the anterior inferior part of the orbit, and beneath the line of the lower lid, a small tumour, which increased in size until a month ago. When she came to the hospital the condition of the eye was as follows:—The ball itself was slightly protruded and nearly motionless, turned upwards and inwards, and completely concealed by the upper eyelid, which drooped over it, and the veins in which were enlarged and the skin of a dusky hue. The eyeball was discoloured, but the cornea was clear; the iris was of a dusky slate colour, the lens yellow and opaque, and projected upwards against the back of the cornea. The conjunctival vessels on the globe were large and tortuous; bulging out over and depressing the lower lid was a large red tumour, composed of conjunctiva. When the finger was pressed on the lower eyelid a large, firm, round mass could be felt and grasped by the finger and thumb, and when so grasped and moved the eye participated in the motion. The woman had the peculiar yellowish skin and look so often characteristic of malignant disease. There was no gland enlarged, and the diagnosis was, that the case was one of melanosis (in consequence of, or altogether with, choroiditis), commencing in the choroid, bursting through the sclerotic, and finally encroaching on the orbit and pushing the eye-ball upwards and inwards. The entire mass was removed by Sir William Wilde last week, and the case had gone on very satisfactorily. The specimen presented the characters of melanotic disease, the interior of the globe was filled up completely with a soft black mass, the retina and choroid being perfectly broken down and unrecognizable; the lens was small, opaque, and close up against the iris, which structure lay in apposition with the



back of the cornea. At the inferior part of the globe the sclerotic presented a funnel-shaped opening, through which the mass in the vitreous chamber was continuous with a firm lobulated tumour, situated external to the globe, and extending chiefly downwards and forwards. The optic nerve, which was partially surrounded by this tumour, was quite black and disorganized; the external tumour was firm and grey with black deposits scattered irregularly through it. Spindle-shaped nucleated cells, large black globular cells, and granules were largely developed in the tumour.—*January 21, 1865.*

*Aneurism of the Aorta.*—DR. HAYDEN brought under the notice of the society an example of aneurism of the arch of the aorta, taken from a female, aged thirty-one, who was admitted into the Mater Misericordiæ Hospital on the 2nd of January, and died on the night of the 15th of that month. All he could learn of her history was the following:—About nine years previously she had an attack of hoarseness and loss of voice, from which she quickly recovered, and enjoyed comparatively good health up to the 21st of last December. On the night of that day she was exposed to cold, and on the following morning found she had again lost her voice; she had also cough, and for this she was treated in one of the city dispensaries with very little beneficial result. The hoarseness and cough continuing, and her strength failing, she sought admission into hospital. Her appearance indicated great distress, and she was haggard in the extreme; her face was pallid, with a tint of lividity; there was complete aphonia, with loud stridulous breathing, audible at a considerable distance from where the patient lay. On the slightest movement of her body, or on exposure of the chest for the purpose of examination, a fit of spasmodic cough ensued, accompanied by copious mucous expectoration of a frothy character. The right radial pulse was 108, weak, but regular; the left was entirely suppressed, as likewise the left brachial pulse, as he thought, on that occasion; there was no dysphagia. She was unable to lie down, and on attempting to do so a fit of difficult breathing came on. With the symptoms before him, and the history he had received of the case, he had in his mind a suspicion of intra-thoracic tumour. He made a careful examination of the chest, but detected no signs to confirm this suspicion, except that respiration in front and under the left clavicle was more feeble than on the opposite side, and also that a shade of dulness on percussion was perceptible on both sides. The bronchial stridor was so intense that it was next to impossible to make a stethoscopic examination of the chest. The case went on from day to day without any improvement. He found that, on the 3rd of January, the day after her admission, he had made a note that she swallowed in his presence some beef-tea with bread soaked in it, a fact to which he wished to direct special attention. On the 7th of



January, on entering the ward he noticed a marked contraction of the left pupil, which continued even in the shade. He made a careful examination of the larynx with the laryngoscope; it was found perfectly healthy, and both vocal cords moved freely in respiration. At this time he asked his colleague, Dr. Hughes, to see the patient with him. Dr. Hughes made a most careful examination of the patient's chest, and declared his opinion that, with the exception of the physiological symptoms of contracted pupil and suppressed pulse, there were no physical evidences of intra-thoracic tumour. This was Dr. Hayden's opinion, notwithstanding that he found dulness on percussion at both sides and above, and feeble respiration on the left side. On that occasion Dr. Hughes' attention being directed to his (Dr. H.'s) suspicion, ascertained that there was very feeble pulsation in the left brachial artery at the bend of the elbow. This was an important fact as distinguishing between the two causes for suppression of pulse, namely, an abnormal distribution of the arteries and arterial obstruction. It settled the point affirmatively as regards the latter, showing that the suppression of the pulse was not due to an unusual distribution of the arteries. On the following day he found there was partial ptosis on the same side; the patient's eyelid drooped so that the equator oculi was reached by the lower margin of the upper eyelid. At this time it was noticed that during the fits of spasmodic dyspnea and cough, which were caused by any movement of the patient's body or an exposure of her chest, pus was discharged in large quantities; it gushed from the mouth on these occasions, followed by momentary relief. The pulse became weaker, the respiration more rapid; and, notwithstanding that the patient took food in moderate quantities, she rapidly sank, and expired on the night of the 15th, dying in a paroxysm of dyspnea.

The examination of the body afforded a satisfactory explanation of all the symptoms, as well negative as positive, that had marked the case. On opening the chest it was found that both lungs were emphysematous. The left was adherent to the parietes of the thorax by recently effused lymph, and its base was in a state of pneumonic consolidation, and from the surface of a section of it pressure forced out purulent matter in considerable quantity. The larynx was perfectly healthy. There was a deposit of fat on the surface of the heart; the valves were healthy; the left ventricle contained a quantity of decolourized fibrin. A large aneurismal tumour sprung from the transverse portion of the arch of the aorta, to the left of the arteria innominata, and extending to the origin of the subclavian, which was pressed upon for one inch from its origin; the tumour sprang from the upper portion of the arch, involving the superior and posterior walls of the vessel; it extended back to the vertebral column, and turned from the left side to the right, passing through the posterior mediastinum behind the trachea, which escaped pressure, and carrying with it the œsophagus,

which was stretched over the tumour and carried with it into the right pleural cavity. The appearance presented by the parts was exceedingly well represented in the drawing by Mr. Conolly which he now exhibited. The aneurism, which had received the impression of the spinal column, was of the size and figure of a large peach. The left carotid sprang from the tumour, but escaped pressure; the left subclavian was compressed and all but obliterated as far as the origin of the left vertebral artery; and thus was afforded a satisfactory explanation of the all but complete suppression of circulation in the left upper extremity. The œsophagus, carried from left to right on the tumour, admitted the finger to be passed down its canal with great facility; from the tumour it passed downward, and to the left side, into the posterior mediastinum. The pneumogastric nerve on the right side was stretched by the tumour; the left recurrent nerve passed from the pneumogastric round the tumour, in which it became so involved that it was impossible to dissect it beyond half-an-inch over the aneurism—so that this nerve was carried from the left side across the posterior mediastinum to the right pleural cavity. It must have been greatly put on the stretch, and afforded a satisfactory explanation of the paroxysmal dyspnea and laryngeal symptoms exhibited during life.

The points of interest in the case were these:—First, the occurrence of the disease in a female, which was rather a rare circumstance; secondly, from the history of the case, the age of the woman, the smallness of the tumour, and the absence of erosion of the vertebræ, it was evidently of short duration; thirdly, that the position of the tumour afforded a satisfactory explanation of the several symptoms, negative as well as positive. They were unable to detect any conclusive signs of aneurism in front, because it had passed to the posterior wall of the thorax, and been overlapped by the emphysematous lungs anteriorly. Hence, the absence of bruit, of pulsation, and of the usual physical signs. There could be little doubt that if he had been able to institute a satisfactory examination of back of the thorax he would have discovered pulsation, and probably the evidence of aneurism, over the superior dorsal vertebræ, against which the tumour lay; but the loudness of the stridor was such that it was impossible to ascertain anything by the stethoscope. The position of the tumour explained satisfactorily the state of the pupil. It must have made pressure on the sympathetic nerve in its descent into the thorax on the left side; it must also have compressed the vertebral ganglion against the transverse process of the seventh cervical vertebra. How was the ptosis on the left side to be explained? In all the experiments of dividing the cervical sympathetic which Bernard had performed, and in those he had performed himself on rabbits, it was observed that ptosis always occurred on the same side as contraction of the pupil. He thought it probable, therefore, that both these phenomena were caused by paralysis of the sympathetic nerve.

There can be no doubt that the attacks of paroxysmal dyspnea, which were brought on by excitement of the circulation, were due to laryngeal spasm, the consequence of increased tension of the left recurrent nerve. It will be recollected that this nerve encircled the aneurismal tumour, and was incorporated in its wall; it must, therefore, have been subjected to increased tension by the temporary expansion of the aneurism resulting from excitement of the heart's action. In the intervals between these paroxysms respiration was easy and unattended with stridor; and, as already mentioned, laryngoscopic examination showed that at these periods the vocal cords moved harmoniously and regularly on both sides in respiration.—*January 21, 1865.*

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PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.\*  
TWENTY-SEVENTH ANNUAL SESSION.

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DR. CHURCHILL, President.

*Placenta Previa.*—DR. TELFORD related the following case, and exhibited the morbid specimen:—Rose Serins, a healthy-looking woman, aged twenty-six, was admitted to the Lying-in Hospital, March 5th, 1865, pregnant of her first child. The menses last appeared on the 15th of last June. At 5 a.m., on the morning of admission she was attacked by hemorrhage; the amount lost the patient cannot state. She became alarmed, and came to hospital, where she remained till 11, a.m., when, not being in labour, and the hemorrhage not having recurred, she went out. She was re-admitted at 9 p.m., of the same day, having had some slight loss during the interval. On a vaginal examination the os was found undilated. There was no uterine action; pulse 80; fetal heart audible. Ordered to remain in the horizontal position. At 2 a.m., on the 6th, a smart gush of hemorrhage took place, from which she lost from eight to ten ounces of blood. On examination the os was found slightly dilated; no presentation discernible. An attempt was now made to rupture the membrane, which was unsuccessful. On slightly dilating the os the placenta could be distinctly felt covering its surface. The nature of the case being now evident, plugging was resorted to, by the means usual in the hospital, namely, a large conical sponge. 10 a.m.—The plug was removed, a small coagulum adhering to its upper surface, but no evidence of recent hemorrhage. Os about the size of a shilling, firm, and undilatable; pulse, 84. The vagina was carefully syringed with cold water containing some of Condy's disinfectant fluid, the plug washed and

\* These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

re-applied in the hope of checking hemorrhage, and inducing uterine action. The patient was ordered 2 ounces of wine, chicken broth, and arrowroot. At 3 p.m. the plug was removed, washed, and re-applied, no progress having been made. 7 p.m.—Ordered 4 ounces wine, and anodyne draught at bed-time. At 10 a.m. on the 7th the plug was withdrawn, and was not re-applied, no hemorrhage having taken place. Os in the same state; pulse 108, rather feeble. Ordered a saline aperient, wine 4 ounces, and chicken broth. 8 p.m.—The pulse had suddenly risen to 130; very feeble; tongue moist and furred; tenderness over uterine region; bowels moved by aperient draught. Ordered 4 ounces wine, a full anodyne, and to have a large linseed-meal poultice over the uterus. 12 p.m.—Hemorrhage to 8 ounces; plug re-applied. Ordered a second anodyne draught. At 10.30 a.m. on the 8th, plug withdrawn; no hemorrhage on its removal; os in the same state; pulse, 130, very weak; tongue moist and furred; lips dry; thirst urgent; stomach irritable; respiration tranquil, 20 in the minute; countenance anxious, and the decubitus that of one suffering from great exhaustion; fetal heart audible. 7 p.m.—Pulse 140; tongue dry and coated; thirst and irritability of stomach continues; complains of severe pain over uterus; fetal heart inaudible. Ordered 2 grains of calomel, with half a grain of opium, every four hours; brandy and a bottle Seltzer water; to have the abdomen covered with a linseed-meal poultice, wet with anodyne liniment. 9th, 10.30 a.m.—Countenance haggard; pulse 140, small and feeble; tongue dry; lips covered with sordes; vomiting of a greenish watery fluid; os a little larger than a shilling; vagina hot. To continue the pills, and to have a mixture containing prussic acid, Battley's solution of opium, and carbonate of soda; brandy and Seltzer water. 1 p.m.—Slight flooding, checked by the application of cold. 3 p.m.—Hemorrhage to the extent of 10 ounces having occurred, the plug was re-applied, the os being then about the size of a halfpenny; pulse 150, very weak; stimulants freely administered. 5 p.m.—Some blood appearing by the side of the plug, and the patient evidently sinking, it was deemed advisable to attempt delivery. The os, though not larger than a half-crown, was soft and dilatable. The pulse was imperceptible in the left wrist, but could be felt in the right, very rapid and weak. The fingers were gradually inserted through the os, and the hand was passed through the placenta, a knee laid hold of, flexion performed, and the delivery completed without much difficulty. During the operation the patient had 12 ounces of brandy. The respiration now became hurried and blowing, the pulse imperceptible. She gradually sank, and died three-quarters of an hour after delivery.

*Autopsy* seventeen hours after death. The body was well nourished, and did not appear very anemic. On opening the abdomen there was no evidence of inflammation found; the peritoneum was quite healthy, as



were all the thoracic and abdominal organs examined. The uterus was laid open by a T shaped incision, when the placenta was seen firmly attached to the lower part of its posterior wall, a large portion hanging down over the os, where it was torn by the hand of the operator.—*11th March, 1865.*

*Practical Remarks on Puerperal Insanity.* By DR. LALOR.

Thirty-nine women, certified as labouring under puerperal insanity, have been admitted to, and came under my observation in, the Richmond District Lunatic Asylum, since I took up my residence there, as resident physician, in November, 1857; the entire number of female cases admitted in same period being 661. Nineteen cases were chronic at time of admission, being of the duration of ten years in one case, four years in one, one in two; ten months in two, nine months in one, six in one, four in two, three in two, two in four, and one in three. Twelve of the chronic cases were cases of mania, and seven of melancholia; eight have been discharged recovered, two relieved, and two died. The deaths occurred in one case from apoplexy, at the age of fifty-four years, eighteen years after the commencement of insanity, and after seven relapses; the other death resulted from phthisis, at the age of thirty-two years, and seven years after commencement of insanity.

Seven of the chronic cases still remain in the institution, as of unsound mind. Of the cases of recovery, three were cases of melancholia, out of a total of seven; and five cases of mania, out of a total of twelve. In another case, which was of three weeks' duration at time of admission, mania did not set in until three months after childbirth; this case still remains in the institution, weak-minded, and labouring under great impairment of memory. Adding this to the nineteen chronic cases, we have twenty cases, which I propose to exclude from further consideration, as I think it will be better to confine my remarks hereafter in this paper to the remaining nineteen, *which form a natural group of acute cases of a first attack of puerperal insanity, occurring within the month after childbirth, and not of more than one month's duration at the time they came under my observation.* The ages of these nineteen cases were as follow:—Twenty to twenty-five years in six cases, twenty-five to thirty in eight, thirty to thirty-five in four, and thirty-six in one case. Sixteen were married, and three single. Ten were returned as of no occupation, which in the case of nine of them, who were married, may be taken to signify that they were occupied solely with their household affairs; five were dressmakers, one a servant, one a bootbinder, one a laundress, and one a marine store dealer. Thirteen were from the city, and six from the county Dublin. Six of the cases were primiparæ; in six it was the second childbirth, in one the third, in two the fourth, in one the fifth, in two the sixth, and in one the ninth.

The form of insanity was mania in sixteen and melancholia in three cases. The duration of the disease at time of admission was—twenty-one days in two cases, eighteen in one, fourteen in two, twelve in two, ten in one, nine in one, eight in three, seven in four, five in one, and two in two. Fourteen were discharged recovered, and are believed never to have relapsed; one was discharged relieved, re-admitted, escaped whilst convalescent after her re-admission, and was kept at home by her friends as recovered. Thus fifteen may be considered to have recovered. Four died. The three cases of melancholia all recovered.

The duration of the disease in those recovered, reckoned from the date of commencement of attack to the date when they were discharged, was—in one case, 316 days; in one, 260; in one, 238; in one, 231; in one, 203; in one, 104; in one, 68; in one, 65; in one, 60; in one, 57; in one, 43; in one, 42; in one, 39; in one, 38; and in one, 34.

The length of residence was—in one case, 302 days; in one, 258; in one, 231; in one, 221; in one, 189; in one, 96; in one, 60; in one, 53; in one, 47; in one, 45; in one, 35; in one, 33; in one, 31; in one, 27; and in one, 26.

On admission, or soon after, I noted the state of the pulse in sixteen cases. In seven cases it was 120, and of these four died; in two it was 104; in two, 100; in two, 96; in one, 80; and in two, 72. I noted the state of the tongue in eleven cases; in five it was clean and moist—one of these died; in one, furred white; in two, dry and coated; in one, moist and slimy—this was a death; in one, dry and furred; and in one slightly furred and dry—this case was a death.

I noted the state of the skin in thirteen cases. In nine it was natural, and of these three died; in two, cool; in one, hot; and in one the hands were moist and warm; the rest of the skin natural in temperature—this case died. Jactitation sometimes occurred; and here, as in other diseases, was a very grave, but not necessarily a fatal, symptom. Wakefulness was a very general symptom. Œdema of the feet occurred in one case, and was followed by convulsions and death. Restlessness, great tendency to movement, excitement, talkativeness, and delirium of a lively character were, on the whole, the most usual indications of the state of the mind; sometimes a remarkable tendency to sing, or to throw words or sentences into rhythmical order, and repeat them with rhythmical cadence, was present. Sometimes a wish to destroy the child existed. Amatory desires were frequently manifested, and indecent acts and language practised. In some of those, and in other cases, there was a disposition to throw off portions of their clothing, so that dresses secured by the lock used in asylums and hospitals for the insane became necessary.

The following is a short summary of the general treatment:—Sedatives were frequently required to procure sleep, especially when the pulse

was quick, and muriate of morphia proved in most cases the best narcotic, in doses of from half to one grain, or more, according to circumstances; less than half-a-grain being seldom efficacious. Hyoseyamus was sometimes tried as a sedative; and, if the tongue was also coated, was given, in combination with blue pill, five grains of each at bed-time, generally with benefit. Tonics, stimulants, and nutritious food were generally called for, and especially when the pulse was quick. Depletion, local or general, and lowering treatment were avoided. When the bowels were constipated moderate aperients were administered, such as mild doses of castor oil, plain or combined with tincture of rhubarb; or aloes were prescribed, in the form of the compound decoction or of pill. When the complexion was anemic, iron proved the best tonic—the *mist. ferri. aromat.* being the best form. Quinine was also frequently and usefully given. Blisters to the nape, vertex, temples, or behind the ears were used—rarely, however, and only for the same indications as point to head mischief in bodily disease. In puerperal, as well as other forms of insanity, the grand rule for medical treatment appears to me to be, that we should be guided by the bodily symptoms, and that the same medical measures are indicated as if the same state of body were met with in a sane person. Refusal to take food occurred here as in other forms of insanity, and required to be met promptly, as weakness and debility were the usual accompaniments and greatest obstacles to recovery. Food was given three times, or at least twice, daily. I used the vulcanised India rubber syphon and tube, recommended by Dr. Tuke in his excellent paper in the 23rd and 24th numbers of the *Journal of Mental Science*; and I consider this the best mode of forced alimentation in cases of insanity, whether puerperal or otherwise. In puerperal mania, as in other forms of insanity, mechanical restraint should be avoided. Cutting the hair was not practised, unless when required for the purpose of blistering; and it is a measure which I think should be avoided in the treatment of insanity, unless there is some positive indication calling for it, which, in my mind, is very rarely the case. The loss of the hair is acutely felt by women in general, and not less so by females recovered from insanity, as being indicative of a disease to which so much degradation usually attaches. The stay in hospitals for the insane after convalescence from insanity is sometimes prolonged to allow the hair to grow, as a necessary condition in order to gain employment, or for other reasons; and therefore I think cutting the hair should be very seldom resorted to, and should be the exception rather than the rule. In convalescence from the acute form of puerperal insanity I believe it is quite safe to discharge the patient after a very short probation, once the restoration to reason appears complete and the bodily health good. I believe that relapses are very infrequent under these circumstances; and the anxiety of the patient and of her friends for a return to home is



generally very strong, and a refusal to gratify wishes so natural would be inadvisable when the danger of evil results from discharge is very small, and less in my mind than from the danger of detention. The caution I have given as to cutting the hair applies, then, with particular force in acute puerperal mania, owing to the chance of discharge being advisable before the hair, if cut, could be well grown.

The important question of prognosis is one of the deepest interest in acute puerperal insanity; and it may not be without use to examine this question here in connexion with the cases just considered, and with the bearing of their results on medical opinion in general, as well as that of some very high authorities on this subject. Dr. Gooch, in his admirable essay on the Disorders of the Mind in Lying-in Women, published in 1831, says:—"I remember the time when it was the prevalent belief among medical men that these diseases were never fatal." The same author also states "that Dr. Baillie, when consulted about a case, is stated to have remarked that the question was not, 'whether the woman was to get well,' but 'when she was to get well.'" The patient referred to died within a week. Dr. Gooch further relates, "that Dr. William Hunter, in his manuscript lectures, said—'Mania, in the course of the month after lying-in, is not an uncommon appearance, but of that species from which they generally recover. When out of their senses, attended with fever like paraphrenitis, they will, in all probability, die; but when without fever it is not fatal.'" Dr. Gooch adds:—"Putting together this statement of Dr. Hunter with my own experience, I extract from it the following meaning: that there are two forms of puerperal mania, the one attended by fever, or at least the most important part of it, a rapid pulse; the other accompanied by a very moderate disturbance of the circulation; that the latter cases, which are by far the most numerous, recover; that the former generally die."

In the general summary which I have given of the results in nineteen cases of acute puerperal insanity of the class defined by me, I have included all cases that came under this class, whether they were simple or complicated with other diseases, as I was anxious to place before the Obstetrical Society some notice of all the cases which came under my observation from their admission to the asylum, in order not to deprive the members of any information coming within the compass of a short paper such as this must necessarily be. I do not think, however, that it would be a scientific treatment of the question of prognosis, as it regards puerperal insanity solely, to include cases complicated with other serious diseases. The cases which recovered were not so complicated. In one of the cases which died, M. A. M., emaciation was present, as were also the other symptoms, as well as the signs, of advanced pulmonary consumption, of which disease she died. That she had consumption on admission was the opinion of Dr. Cruise, who recommended her to the asylum, and also



of Dr. Tuohill, one of the consulting physicians of the asylum, and my own. In a second case of death, that of E. M., puerperal metritis and peritonitis was evidenced, and recognized during life by Dr. Byrne, who recommended her for admission, by Dr. Banks, and myself,<sup>a</sup> and was proved, by *post mortem* examination, to have existed.

In a third case of death, that of F. F., there was œdema of the feet from the first, and the œdema subsequently extended up the legs and thighs; the complexion was waxy; the secretion of urine scanty; and convulsions and hemiplegia preceded the fatal result. These symptoms do not belong to uncomplicated acute puerperal mania, but are well known to accompany renal disease very frequently. In this case also a dry brown tongue and sordes on the lips set in about two days after admission, and indicated more fever and of a different type than the fever of paraphrenitis referred to by Gooch. I, therefore, concluded that renal disease existed and gave rise to uremia and serous effusion which were the causes of the unusual symptoms narrated, and of death. Thus in two out of the four cases which died serious complications existed beyond all question, and it was to these diseases and not to puerperal insanity that death was attributable. In the third case I submit that there were sufficient grounds for holding the same opinion. Abstracting those three cases from the seven in which the pulse was rapid, we have four cases of uncomplicated acute puerperal mania of the class under consideration with rapid pulse, but with no other grave febrile symptom. Of these, three recovered and one died. This is equal to a mortality of 25 per cent., and proves the great value of taking into consideration the rapidity of the pulse as an important guide in forming our prognosis.

The range of pulse from 100 to 104, noticed in four cases, may be considered something more than what Dr. Gooch calls a very moderate disturbance of the circulation, and may be taken to represent a class of cases intermediate between those with the pulse indicated by that expression and the pulse implied by the term "rapid pulse." No death occurred, and recovery followed in all those cases just as in those where the pulse did not exceed 96, or the rate which is generally understood to be indicated by the term "moderate disturbance of the circulation." The general proposition as to the absence of danger to life, and the almost absolute certainty of recovery of reason in cases of acute puerperal insanity, attended by little disturbance of the circulation, as laid down by Gooch, agrees with my own experience. Further, abstracting these cases with serious complications from the entire nineteen cases under consideration, we have remaining sixteen cases of acute uncomplicated puerperal mania; and of these fifteen recovered and one died, being at

<sup>a</sup> A very graphic report of this case, by my friend Dr. Banks, is published in the Dublin Hospital Gazette, for December 15, 1859.

the rate of  $93\frac{1}{6}$ th per cent. of recoveries, and  $6\frac{1}{2}$ th per cent. of deaths. I believe that at the present day there is no such prevalent belief amongst medical men as was held in Dr. Gooch's recollection (and indicated by the saying of Dr. Baillie which I have quoted), viz.—“That disorders of the mind in lying-in women are never fatal.” If any one retains such a belief, the results just stated are amply sufficient to disprove it, and my paper will not be entirely useless if it should remove a very serious error of opinion from the mind of a single practitioner of medicine. At the same time, if my later amended analysis, confined to cases of uncomplicated puerperal mania, offers a more legitimate deduction from fitting facts than that presented by my first statement, of the gross results in all cases, whether simple or complicated, a more hopeful view may be taken of the disease in that form which I believe was alluded to by Drs. Hunter and Gooch, in the propositions which I have brought under the notice of the society, than was put forward by those eminent authorities in these propositions. Nevertheless, acute puerperal insanity, occurring within the month after child-birth, even when uncomplicated and brought under treatment within a month after its commencement, should not be considered otherwise than as a very serious disease; and I consider the results I have just stated in sixteen cases as, on the whole, satisfactory. That they were so satisfactory is, I think, attributable to the discriminative administration of nutritious food and stimulants, and to a like avoidance of lowering and depleting measures. This was the plan of treatment recommended by Dr. Gooch himself, and generally followed at the present day, and in the advocacy of which I do not assume to be at all peculiar, or to have any other merit than that of following the track which the general experience of the profession has pointed out as the best and safest. In writing this paper my chief object and hope have been to stimulate inquiry, especially amongst the junior members of the society, rather than to satisfy it; and it is my anxious desire to induce them to direct their attention more than is usual to the study, not alone of the very interesting form of insanity which has been the subject of this paper, but of insanity in general, of which it forms only a very small portion, as may be learned from the fact that the number of cases of puerperal insanity in Ireland, on 7th April, 1861, according to the last Irish Census, was only 37, whilst the number of cases of insanity from all causes, at same date, was 14,098.

The many great discoveries made by members of the medical profession in the anatomy and physiology of the nerves and nervous centres, have partly drawn the science of mind out of the labyrinth of metaphysics. The general dependence of mental disturbance on morbid bodily action, suggested by anatomical and physiological research, has been confirmed by medical observation of the disorders of the human body during life and after death, and hence, the treatment of insanity has become a

recognized branch of medicine. The foremost rank now held by medicine in psychology, theoretical and practical, was only acquired after a long and arduous struggle with rival claimants belonging to other professions, or to none, many of them being persons of large minds, varied acquirements, of much worldly knowledge and learning, or of great benevolence; these qualities being sometimes more or less in combination. But I believe it will be admitted that the study of psychology cannot be fully and successfully cultivated without the aid of those scientific methods which are the main sources of all medical knowledge. It follows, that as long as the healing art shall have existence so long shall the treatment of insanity be our right. It is, therefore, our duty to acquire the knowledge necessary for its correct practice; and the education of the medical student ought not to be considered complete until it has embraced the study of so-called "mental disorders."—11th March 1865.

*Dropsy of Fallopian Tubes.*—DR. BENNETT exhibited to the society a recent specimen of this. He said:—The specimen which I bring before the society this evening was taken to-day from the body of a middle-aged woman, brought for dissection into the Medical School of Trinity College. I discovered the tumour on opening the abdomen and turning aside the intestines; it occupied the cavity of the true pelvis, filling it completely, and projected upwards a short way above the brim. The tumour at first sight looked very like a distended bladder; its walls were, however, much thinner than those of the bladder. It was free from adhesion either to the intestines or walls of the pelvis, but could not be raised in the least out of the pelvic cavity, in consequence of the shortness of its pedicle and the intimate connexion of the pedicle to the uterus. In order to make out the nature of the tumour exactly it was necessary to remove it with the bladder and uterus. I now exhibit to the society the parts so removed. The tumour is a very peculiar one—its shape ovoid; the broad end, that remote from the uterus, about the size of an infant's head; the narrower end is in contact with the fundus of the uterus, and has coiled around it spirally from left to right a tube which, on examination, proves to be the Fallopian tube dilated. The tumour is evidently a simple cyst, and its cavity communicates freely with that of the dilated Fallopian tube, which is lost in the tumour, there being no part of its fimbriated extremity discoverable; the broad ligament of the uterus contains the tumour, and attaches it to the uterus. The ligament of the ovary passes directly into the cyst wall, and all trace of the ovary, external to the cyst, is wanting. There are a few adhesions about the attached part of the tumour, between adjoining parts of the broad ligament. On opening the cyst the fluid contents are found to be ordinary serum, the cavity is proved to be a simple one, and where the Fallopian tube is lost on its surface, externally, a finger can be introduced

from the cyst into it for some distance; the tube gradually contracts, and is completely impervious two inches from the uterus; in no part of the tumour can the ovary be detected, though in the part of the cyst wall which contains the ovarian ligament the remains of the vessels of the ovary can be seen. The condition of the uterus and the parts in the left broad ligament, explains, I think, the nature of this tumour. The os uteri is so contracted as barely to admit an ordinary probe; the tissue of the cervix and body of the uterus is much harder than usual, and their cavities are greatly contracted. Both Fallopian orifices are closed; the left tube, like the right, is impervious for some distance, and then dilates into a thin cyst, which retains the shape of the tube pretty accurately. The extremity of the tube is closed, its fimbriæ are gone, and it is intimately attached to the ovary by two adhesions—one, the normal one, thickened, the other abnormal. The ovary is free from any trace of disease, cystic or otherwise, and is such as we usually meet with in women past child-bearing. From such a condition of parts I think we may deduce the cause and nature of the larger tumour, even without a knowledge of the history of the case during life. The condition of the uterus and left ovarian structures shows that an attack of inflammation, probably catarrhal, caused contraction of the os uteri, obliterated the left Fallopian tube at either end, and caused it to adhere abnormally to the ovary. On the left side the progress of disease was arrested, while on the right the same attack of inflammation, having caused similar adhesions between the ovary and its duct, was succeeded by cystic growth; the extent of growth of the cyst formed thus by inflammatory adhesions, has involved the ovary with its ligament and vessels, and finally destroyed all trace of the ovary.

This form of dropsy differs in many points from those which occur in the ovary itself, or between the layers of the broad ligament, which seem to owe their origin to cystic degeneration alone, without any inflammatory process—the latter to cystic degeneration of the parovarium, the former to the same change affecting a Graafian vesicle. If my deductions from the facts of the case are correct, the previous occurrence of inflammation of the uterus and its appendages would be necessary in order to the formation of such a tumour as this before us. The occurrence of such inflammation, and of the consequent condition of the uterus, taken with the fixed character of the tumour, might assist in the diagnosis of this form of dropsy.—*11th March, 1865.*



TRANSACTIONS OF THE COUNTY AND CITY OF CORK  
MEDICAL AND SURGICAL SOCIETY.<sup>a</sup>

SESSION 1864-65.

DR. JOHN BAIN, President.

*Bronzed Skin.* By DR. POPHAM.

DR. Popham submitted to the society the kidneys and supra-renal capsules, with a section of the skin, taken from the back of the neck of a woman, who had died of supra-renal melasma. Ellen Donovan, aged 50, unmarried, was admitted to the Cork Union Hospital, January 22nd, 1864, with bronzed skin, which had occurred twelve months previously, after jaundice. Her olive-coloured appearance would strike any person who saw her. The back of the neck next the hair was as dark as the hue of a negress. The whole of the back was a deep brown, very marked about the hips; the anterior surface of the trunk was less tinged than the posterior. The upper part of the neck and thorax were thickly dusted with white furfuraceous scales. The colour of the arms was lighter than that of the body, the dorsum of the hands being the deepest part, and the palms of natural colour, while the nails had lost their pinkish tints. The nipples and areola were as dark as in the pregnant state; but the circle of the areolar nigrities scarcely exceeded an inch diameter, and it was distinguished from that of pregnancy by the absence of tumid follicles. The face was more sallow than brown, but the presence of inky spots on the prolabium at once fixed the attention; these patches were on the inner surface of both lips, most on the upper and along the margin of the gums, and more sparsely over the buccal mucous membrane; the tongue was pale; the conjunctiva was discoloured; one eye suffered from cataract, the other from opacity. The legs retained their natural colour; the surface of the skin felt dry and rough—no itchiness or soreness; the application of a blister always developed an increased amount of pigment.

This case continued under Dr. Popham's observation during fifteen months, and scarcely any improvement in the colour was observed in that time; the only external application of any good was lemon-juice; the internal use of this medicine also promises some advantage. The general symptoms were, however, by no means stationary. Her appetite was at first voracious; but during the last month it was abolished altogether; no vomiting occurred till towards the close, when it became uncontrollable; diarrhea, at the beginning frequent and with orange discharges, passed finally into obstinate constipation. The pulse rose slowly from 72 to 120, and the heart-stroke and sounds, became gradually weaker; thirst was almost the only symptom which suffered but little

<sup>a</sup> These reports are supplied by Dr. David Cremen, M.B., T.C.D., Secretary to the Society.

change; she never complained of pain. The urine was greenish-yellow, in fair quantity, clear, acid, sp. gr. 1014, average, with neither albumen nor sugar; nitric acid caused a copious purple deposit. All through she had painful depression of spirits. Phthisical symptoms set in at the close, but the most distressing complication at that period was the incessant vomiting of blackish and acid matters. Pediculi existed in enormous quantity. The mind gave way at the end, and delirium, moaning, and screaming disturbed her last moments. She died, April 30th, 1865; the discolouration having continued twenty-seven months.

The *post mortem* was obliged to be hastily conducted. Emaciation had set in with the vomiting, but still was much less than in ordinary phthisis; no change by death in the colour of the skin. On comparing the two renal capsules, the right appeared but little changed, but the left was much altered—it was pulpy and diffuent, wasted in size, and contained a cavity the size of a pea with unhealthy pus; no cheesy or gritty matter existed. The kidneys appeared normal; liver fatty; tubercles were found in the lungs; the heart flaccid and pale.

Dr. Popham has seldom seen a case in which the *tedium vitæ* was more marked, or the vital powers so hopelessly and helplessly prostrated as towards the close of her life.

*Case of Infantile Poisoning by Tincture of Opium Treated by Extract of Belladonna.* Reported by DR. S. O'SULLIVAN, House Surgeon, North Infirmary.

Daniel Ryan, a child, twelve week's old, was brought to the North Infirmary on the evening of the 22nd April, 1865, at seven o'clock. At four o'clock this afternoon he got, by accident, a dose of laudanum, equal to about one scruple. Very soon after taking it, he started up, as the mother states, in a fit, threw his arms about, vomited, and then sank, as if exhausted. As the child continued in this prostrate condition, the mother took him to Dr. Allen, from whom he got an ammonia mixture, of which one or two doses were administered to him. Soon after he was brought to the infirmary. He was then in a lethargic condition; surface of the trunk of the body warm, lower extremities very much flexed and cold; eye-lids spasmodically closed, pupils contracted to the size of a pin's point. He had not passed any urine since he took the laudanum. Pulse rapid and feeble, could scarcely be felt, intermittent.

Applied cold douche to the spine and sternum alternately. This caused him to gasp for breath, and roused him. Administered strong tea and a solution of tannic acid. He also got a turpentine enema. The attendant was directed to try to keep him from sleeping. About nine o'clock, p.m., Dr. Popham ordered him to get one drop of a solution of three grains of the extract of belladonna to the ounce of water. This dose was repeated three times at intervals of two hours. At three o'clock,

a.m., on the following morning the pupils dilated a little, and he passed water freely. His bowels, which had not been disturbed by the first enema, which was retained, were now well moved, after the exhibition of a small assafetida enema. From this time he continued to improve without showing any other bad symptom. He was discharged on the 23rd, cured.

*A Case of Fracture of the Pelvis, Complicated with Rupture of the Urethra, &c.* By STEPHEN O'SULLIVAN, M.D., House Surgeon, Cork North Infirmary.

Patrick Hennessy, aged twenty, admitted to the North Infirmary on the 15th October, 1864, about half-past seven o'clock, p.m.

Was on an errand this evening delivering books, near the railway station, on the Lower-road, when a horse, drawing a heavy cart, took fright at the sounding of the railway engine whistle, ran away, and knocked the unfortunate young man down. The wheel of the cart passed over his body.

On his admission to hospital there are found several abrasions on the upper and lower extremities; the latter are apparently in a paralysed state, without anesthesia. The right limb appears somewhat everted, and quite powerless. Upon examination, however, there is found to be neither dislocation or fracture of the femur. He cannot bear to be disturbed, the least motion causing him much pain. He lies with most ease on his back.

The right testicle is forced up under that portion of the integument covering the *corpus spongiosum* of the penis. By a little manipulation the testicle is easily reduced to its proper position in the scrotum. Complains of much pain in the back. On examination, a puffy tumour is found over the right side of sacrum.

There are found no external marks of violence in front over the hypogastric region which would lead one to suppose the existence of any internal injury.

His pulse is scarcely perceptible at the wrist (120). Countenance very pallid. Is in a condition nearly approaching to syncope.

Stimulants are administered, but without any apparent effect.

Half-past eight o'clock, p.m.—Complains of violent pain in the abdomen, which continues to increase. Upon inquiry it is found that he has not passed water for some time, and a catheter is gently introduced. The instrument having reached nearly the neck of the bladder, is found not to enter that viscus but to take an unusual turn, and is consequently withdrawn, stained with blood, leaving no doubt of rupture having taken place.

From this time the patient continued to sink rapidly. Death took place at ten o'clock, p.m.

*Autopsy* made October 17th.—Upon opening into the peritoneal cavity, there is found an increased vascularity of the intestines, with one or two marks of ecchymosis. Some blood is found in the lower part of this cavity. The peritoneal covering of the bladder presents an appearance of extreme ecchymosis, which might almost, at first sight, be taken for gangrene. It is of a dark red colour. There is much blood effused beneath it, and on the surface of the bladder, as well as into the cellular tissue of neighbouring parts. Upon the introduction of a catheter (into the penis) it is found to enter the cavity of the pelvis, emerging from the urethra in front of the prostate gland.

The two horizontal rami of the *ossa pubis* are found fractured, and the symphysis is also partly disarticulated and crushed.

A large spiculum of bone from the back part of the symphysis pubis is found to project against the bladder.

There was much obscurity in the diagnosis of the foregoing case, and not without reason, when it is considered that the existence of so much internal mischief was not manifested by any corresponding external mark of violence. The only way in which it might be suspected that there existed fracture of the pelvis was by remembering that the urinary organs had sustained great injury, by a careful observation of the state of the lower extremities, and by noticing the position of the patient.

*On Progressive Motor Ataxy.* By Dr. L. W. SALOMONSEN. Translated from the *Bibliothek for Læger* for January, 1863. By WILLIAM DANIEL MOORE, M.D., Dub., M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

(Continued from Vol. xxxviii., p. 502.)

FEARING to weary the reader by the quotation of too many cases, I shall not here bring forward examples of the many irregularities in its course which this disease, like all others, may present, and of which numerous descriptions are to be met with. I shall only direct attention to the fact that it is especially the first stage which may be irregularly developed, or the symptoms of which may be partly, or wholly, wanting, and not present themselves until later in the course of the disease. The characteristic pains are most rarely absent; but Duchenne records a case in which they did not set in until more than two years after the commencement of the disease. More frequently the anomalies of the eyes, the amaurosis, and the paralysis of one or more of the nerves connected with the eyes, may



be absent in the commencement, but they, too, will always occur at a later period, though it may be only transiently. The same is true of other nervous affections. Only once has Duchenne found the sexual functions wholly unimpaired, although the disease was already in its third stage; and in like manner he has only once seen the abnormalities of urination and defecation limited to slight constipation, although in this instance, too, the ataxia was far advanced. Most rarely of all does the disease begin from the first solely with signs of deficiency of co-ordination of movements; rather more frequently is it the case, on the other hand, that these begin in the arms, and do not, until subsequently, attack the legs. Lastly, there are also examples of the disease occurring hemiplegically, being confined to the arm and leg on the same side of the body; both Duchenne and Moreau (de Tours)<sup>a</sup> have recorded such cases. Not only does the disease thus acquire a quite peculiar stamp, but it is evident that it may thus more readily simulate, and be confounded with cerebral affections, especially with apoplexy, and that the treatment adopted may, consequently, be influenced thereby.

The same remarks which I made above with respect to the symptomatology may here be repeated in speaking of the diagnosis of ataxia, and especially of the distinctive marks between it and the other more or less allied nervous affections, which the researches of modern times have first more accurately classified and defined; and I may here especially refer to the article in the *Ugeskrift*. The observations which I shall now make are, therefore, very few. In the first period the signs are, separately, of course, usually rather uncertain; but when combined they acquire a pathognomonic value, and predict the subsequent development of the disease. Every spontaneous strabismus or diplopia must thus direct attention to the possibility of the presence of the disease; and, even if it is removed by one remedy or another, our suspicions ought not to be lulled: it will often return again, or the other symptoms may be developed without it. Still more does this apply where the strabismus is accompanied by complete or incomplete amaurosis. Each affection separately is, of course, of less importance; combined, they are much more serious. Usually only one eye is affected, and precisely for that reason the patient may easily fail to attach much importance to the symptom, or he may even himself be unconscious of its existence; too often, however, both eyes are attacked, and sight is completely lost, and that even early in the course of the disease—as, for example, in Charcot's and Vulpian's patient. Affections of the genital organs have about the same importance. If, without demonstrable cause, spermatorrhea, or impotence, should occur in a man still rather young, ataxia ought always to be present to the physician's mind. Thus, for example, Duchenne has

<sup>a</sup> See l'Union médicale, 1862, No. 122.

seen a case where the patient, a man of twenty-eight years of age, consulted him for impotence, which had commenced a year previously; his look, however, struck Duchenne as remarkable; and, on closer inquiry, he found that he had, during the last two or three years, occasionally seen objects double: on the other hand, he nowhere felt pain. Now, were the impotence and the disturbance of vision accidental combinations, and was each, by itself, a distinct disease, or were they the precursors of ataxia? The future was soon to reveal this; for, in a very short time after the patient came under treatment, he complained that he could not securely balance himself, and soon after the ataxia was very well marked. Still, the characteristic pains are especially important in the diagnosis. Hitherto they have usually been regarded as neuralgic or rheumatic, and have been treated accordingly; now they will, especially when combined with one or more of the above described affections, naturally acquire a much greater importance. Trousseau goes on this point so far as to say that he has never yet seen them in any one who did not subsequently become ataxic. If the anomalies of movement have first occurred the diagnosis is, of course, easier; but even then it may be worth while to attend to particular circumstances. Thus the want of co-ordinating power need not, especially in the commencement, be very striking; and the lesions of sensibility may, even during the whole time, be altogether wanting. It is then chiefly the state of the movements, when they are unaided by sight, which can be a guide to the physician. Even Romberg lays particular stress on this point. He says:—"The patient must, even from the commencement of the disease, be able to see his movements, if they are not to be still more uncertain than under other circumstances. If he be made, while he stands erect, to close his eyes, he begins forthwith to tremble and shake, just as he walks and stands with more uncertainty so soon as it becomes somewhat dark. Thus a patient, whose sight was unimpaired, complained that he found it impossible to stand and wash in his dark sleeping-room; to avoid falling about, he was obliged to dress in a better lighted apartment. Another was obliged, when he went out in winter at six o'clock in the morning, to have a guide both at home and in the street." This symptom Romberg considers to be pathognomonic, for it is met with neither in other paralyzes nor in uncomplicated amblyopia; and he therefore called attention to it so long as twenty-five years ago. Duchenne does not, indeed, lay so much stress upon it, but he shows decidedly that the lesions of co-ordination, although they are always present when the patient can see, become particularly distinct in the dark, or when the patient is not allowed to look before him; and he employs this circumstance especially to distinguish ataxia from the disease which, on less accurate investigation, it most nearly resembles—namely, paralysis of the muscular sensibility. Patients with the latter disease, in fact, provided they can see,

do not exhibit the least want of co-ordination in their movements, but perform them in a perfectly normal manner. Trousseau's practical sense has, on the contrary, led him independently to comprehend and estimate the importance of this fact. In the normal state, he says, persons can both stand erect and walk with closed eyes; but their gait is more insecure than when they walk with their eyes open. No one can walk quite as well with closed eyes; and even if one was assured beforehand that he would meet with no obstruction his steps would become unequal, and he would walk in a quiet, cautious manner. This is the case equally in the most normal condition, and in the purest paraplegia or in ataxia: it proves that even in the most healthy the sight is necessary to control the steps, and to make them even and uniform; and this is the reason why a blind man, long as he may have been blind, must always use a stick in walking, otherwise he would, in fact, constantly miss his direction. Now, he, indeed, incessantly loses it, but the stick brings him quickly back to the right direction. It is, therefore, not in this way that ataxia can be recognized. On the contrary, it may, even at a very early period, be distinguished from every other affection by the following marks:—If we desire an ataxic person to stand erect, with his feet close and parallel to one another, he does it only with a certain difficulty, even if his eyes are open and the disease only commencing. But, if any disturbances in walking have already occurred, it is quite impossible for him to hold himself erect when his eyes are closed; he immediately begins to tremble, and would fall about if he did not either open his eyes and lean on something, or make a great effort to recover his equilibrium. This is pathognomonic, for it is met with neither in the sound, nor in those who are hemiplegic or paraplegic, for example, after apoplexy, in general paralysis, nor, lastly, in the blind. If the disease is a little more advanced the same test may be employed in walking. Even if the patient, when he has leave to look, usually walks without tottering, maintains his balance perfectly, and, for example, complains only of feeling his limbs weak, he will, the moment he closes his eyes, or finds himself in the dark, begin to reel like a drunken man, and he will scarcely be able to walk a step without falling. On the whole, the patient's condition in walking is still less peculiar than that just described as existing when he is in the standing position; for the ataxic gait may, as the examples already quoted will have proved, vary very much: even if the patients are, moreover, perfectly blind their gait need not differ essentially from that of other amaurotics. Some walk slowly; others, on the contrary, quickly, with jerking steps, or they can take only short steps, or, finally, they walk quite stiffly, almost without bending the joints of the legs; but they all reel when they stop, often even if their sight is quite unobstructed, constantly on the contrary, so soon as they are unable to see. The second characteristic mark of the disease I am describing is the preservation, in all its stages, of the



muscular power, which is recognized partly by attempts to perform movements with the limb against the patient's will—partly by the dynamometer—partly, lastly, by investigating what exertion of force his apparently paralytic limbs, which he also himself feels to be weakened, are capable of exercising. Even if the patients cannot walk a step without support, it is often quite impossible to flex or extend the leg against their will; or they can, when sitting, themselves extend it so violently that another person standing opposite to them is thrown down by the blow; and as to the arms, Trousseau has seen patients who, if they only leaned upon something, could carry upwards of ten stone erect upon their shoulder; nay, he has even seen an old man of eighty who could, with ease, lift and carry his physician. The third mark which distinguishes this disease, particularly, from chorea, paralysis agitans, or the other kinds of more or less universal tremors, is, that the irregular movements in ataxia are produced only when the individual desires to perform a movement, while, in the other diseases, they exist also when the patient keeps himself quiet. Lastly, the fourth mark which distinguishes ataxia especially from diseases of the cerebellum is, that the patients affected with such cerebral diseases easily lose the balance of the whole body, and, therefore, reel much in walking and easily fall; while the ataxic patients' gait is difficult only because they cannot direct the movements of their limbs according to their own will. The patients with cerebral disease, on the contrary, do not so readily fall about when they shut their eyes, while they stand erect, as ataxic patients. Frequent vomiting will, moreover, usually accompany the cerebral disease; and all the precursors of ataxy, especially the pains, will be completely wanting.

As to its duration and course, we need scarcely observe that this is, in general, a chronic disease, often even one of the most protracted—that it may last ten, fifteen, twenty years without attaining any great development, even without passing its first stage, and without essentially disturbing the general health. On the other hand, it may, however, also be tolerably rapid: thus it may, for example, even in six months from the commencement, have become general; and the patient is, in such a case, usually so much reduced that the slightest intercurrent disease suffices to carry him off.

Of its etiological elements some are more, others less, well known. All writers agree that it is especially the age of manly vigour—from twenty to forty years—which is most liable to be attacked by it. Women much more rarely suffer from it. Romberg states that their number, in comparison to that of males, is only one-eighth; Duchenne has seen it only three times, Trousseau, among fifty cases, has met with it only four times in women. Still, Charcot's, Moreau's, and Isnard's three patients were all women. Trousseau's example of a man, aged eighty, suffering from it, and Isnard's patient of seventy-two years, show that old age is



not absolutely exempt from it. Onanism, spermatorrhea, syphilis, excesses of various kinds, the suppression of habitual perspiration of the feet, are, further, more or less correctly mentioned by all as the more proximate causes. One element there is, to which Trousseau is the only one to direct attention, and that certainly correctly—namely, hereditariness. This is not to be understood as if this disease—like phthisis, for example—passed as a direct inheritance from father to son; attention has too recently been turned to the point to admit of its having been as yet investigated. What I wish to say is, that the same is true of this as of other nervous affections—that allied forms are met with in several members of the same family—that the whole kindred in general are predisposed to nervous diseases. The latter, as is well known, often change their form; and, nevertheless, we are justified in referring them to the same class, and in recognizing a common bond of connexion between them. Even Isnard's patient will have justified this assertion; the examples quoted by Trousseau establish its correctness still more distinctly. The father of one of the ataxic patients, in his sixty-fourth year, became melancholic, but recovered completely. His sister was an idiot, and of her two children one was paralytic and demented, the other was an idiot. His father's sister was also insane, and one of her sons was epileptic. Another of Trousseau's patients had a brother, who was also ataxic; another brother who was attacked with hemiplegia; while one of his uncles and an aunt were insane. Lastly, this writer mentions a third patient, who had been for more than twenty years ataxic, but, like the others, had never suffered from any intellectual disturbance. His father killed himself; one of his brothers was clear in his intellect, but, under the influence of an irresistible impulse, uttered loud cries; the other brother suffered for many years from singular muscular spasms. These examples of the transmutation of nervous diseases from one form to the other are certainly as striking as possible; and there can be no doubt that the future will exhibit, still more fully, the correctness of Trousseau's opinion in this respect.

Before proceeding to speak of the most interesting point in this disease—namely, the question of its essential nature, pathological anatomy, and physiological pathology—I shall here dwell a little upon its treatment, not because it is one of the diseases in which treatment can accomplish much, but because the hopelessness of earlier writers has been in part shaken, and because the progress of science may be traced in this direction also. Against Romberg's disconsolate assertion, that "every patient suffering from this disease is irrevocably condemned; that every hope of recovery has disappeared, and that the physician ought to confine himself to being as little active as possible; and that he should, in fact, endeavour only to allay the pains by means of veratria ointment, and to regulate the digestion by cold lavements, avoiding evacuations of blood,

derivatives, long continued bath-cures, &c.," even Duchenne goes a little less far in a negative direction. If the disease is fully developed he doubts that any medical treatment can avail; but he hopes that when, hereafter, it can be recognized with certainty in its first stage, it may be arrested in its progress; and he knows undoubtedly, from his own experience, that at least some of its symptoms can be removed, or essentially alleviated, in the beginning, and even later in its course. It is especially local electricity, faradisation, which he has found of essential service. It has removed the strabismus, or the diplopia; has diminished, or temporarily arrested, the violent pains; and has even subsequently essentially lessened the cutaneous or muscular anesthesia, and has, at the same time, again contributed much to remedy the want of co-ordination of the movements, and vastly to improve the patient's gait. But to act upon the source of the disease—at that time (the date of the publication of his essay) still unknown to him—he considers all local means insufficient: a more general treatment is, for that purpose, necessary. If there be a trace of previous, or of still-existing syphilis, mercury or iodine is, of course, indicated; but, even where this definite indication is wanting, Duchenne has seen benefit from the preparations of iodine, and he therefore always combines local faradisation with their use. Of course this treatment is purely empirical, and he consequently states, beforehand, that he has not so much faith in it as to prevent him changing it for any other mode which others may find efficacious. Trousseau, on the whole, agrees with Duchenne, except that he proposes the use of opium, or of belladonna to relieve the pains, of a compressor of the prostate in spermatorrhea, and of turpentine in retention of urine. While he enforces prudence in the use of sulphur-baths, or of the water-cure, he does not deny that these means may sometimes be of use. In one case, treated by Bourguignon,<sup>a</sup> the water treatment, together with electricity, and the internal use of strychnia, really appeared to have cured the disease. In 1861, however, Wunderlich proposed a new remedy—namely, nitrate of silver—in this disease. He was led to try it by the fact that a lady, who suffered from extremely violent hysterical spasms, followed by universal paralysis, both of sensation and motion, and who knew, from previous experience, that this paralysis was rapidly removed by the use of this remedy, was not in the least improved by other medicines which he tried, notwithstanding that he had led her to believe that they contained the salt of silver, while she got rid of her paralysis as soon as he put her upon that preparation. In the course of a short time five cases of well-marked, though not very advanced, ataxia came under his care; in them all he tried this remedy almost exclusively, and in all it had an excellent effect. All the ataxic symptoms were essentially relieved; ordinate, regular movements took place; the gait became

<sup>a</sup> See *L'Union médicale*, 1862, No. 39.

more steady, so that the patients could take long walks, and, without difficulty, go up and down stairs; the pains diminished considerably; the anæsthetic signs were much lessened, or ceased altogether; defecation and urination again became normal; and, in some cases, the previously-extinct sexual impulse even returned; at the same time the weight of the body steadily increased, and the patients' whole condition and appearance improved. He gave it in pills, containing from  $\frac{1}{20}$  to  $\frac{1}{8}$  of a grain in each, increasing from three to six times a day; and, on the whole, the patients got a considerable quantity, namely, from half a drachm to a drachm. Later, he has twice had occasion to try this remedy,<sup>a</sup> and he has likewise seen striking benefit from it. He admits that its effect can be explained neither physically nor chemically; that it is pure empiricism, or induction, which has led him to employ it. He further remarks that the improvement attained is not necessarily lasting, but may, perhaps, prove to be only transitory. But he asserts, and correctly, that in so formidable a disease, and one hitherto believed to be incurable, even a temporary improvement must be considered a benefit. He does not suppose that he can, by this means, remove the changes in the substance of the spinal marrow, which are met with on every dissection of a far-advanced ataxic patient, and he therefore feels confident that the remedy will prove inefficacious in cases where such changes have taken place. But, on the one hand, he believes that the ataxic phenomena may occur without the spinal marrow being necessarily perceptibly altered on anatomical investigation; and, on the other, he does not yet know whether the abnormalities in the spinal cord—even if we admit that they are met with in all cases—are the cause, or a secondary consequence of the disease; and, on these suppositions, we may hope that if we restore the peripheral nervous functions we may also remove the central lesion.

After him several have adopted this remedy. Trousseau, who in his theoretical views on this point partly resembles him, has not yet, indeed, had the opportunity of testing it with sufficient steadiness in this disease; but he has so often seen marked benefit from its use in other neuroses, especially whooping-cough, epilepsy, and hysteria, that he *à priori* places great confidence in it, and warmly recommends its employment. But it has been advantageously prescribed, by other French writers, at least in three cases. Thus, Charcot and Vulpian, in the article quoted above, mention that in two patients they have with it obtained very essential improvement, both in movement and sensation; that the pains have ceased, and that in one case even the sight became better; subsequently they must have successfully used it several times, for quite recently the Académie de Médecine has awarded<sup>b</sup> them a prize of 1,000 francs for a

<sup>a</sup> See Archiv der Heilkunde, 1863. Heft 1, p. 43.

<sup>b</sup> See L'Union médicale, 1862, No. 146.



memoir sent in, upon the use of nitrate of silver in ataxia. This prize has been given them from the Barbier legacy, "which is to be distributed to those who have discovered perfect remedies for any disease hitherto considered incurable, or a remedy which nearly approaches thereto." Theoretically they, moreover, start from the principle, that material changes in the spinal marrow lie, from the commencement, at the bottom of the disease, and produce its phenomena ; but they believe that when it is possible by this remedy to arrest and to remove the morbid process, the altered nerves may be again restored, and their functions more or less completely re-established. They rest this view upon the fact that the dissection, in their patient described above, among other things exhibited regenerated nerve-fibre, inasmuch as it shows that such a restoration is possible, even in a far-advanced stage ; it is therefore much more likely to take place when in the commencement of the disease the anatomical changes are much slighter. Lastly, Moreau, too, has, upon their recommendation, employed the medicine in his patient, and also with excellent effect. In this case the disease had progressed for four years in spite of all the means employed. Under the use of nitrate of silver all the symptoms diminished to such a degree that the patient thought herself cured, and that, in fact, every medical man must consider her state to have been much improved. It was not only the bodily symptoms which were in part removed, but the epileptic fits, with which she had before very often been attacked, and which certainly must rather be looked upon as evidence that the disease had extended to the medulla oblongata or cerebellum than as an accidental complication, remained away all that time, and her weakened faculties, especially memory, improved. In the middle of the treatment Moreau gave her, as a test, without her knowledge, for fourteen days, pills consisting only of crumbs of bread ; the improvement which had already begun ceased, and her state became immediately aggravated. Directly after the nitrate of silver was resumed, on the contrary, the improvement again went on rapidly. As a proof of the impregnation of the system by the medicine, he calls attention to the fact that not only does the skin often become of a leaden colour, but that the gums also readily become bluish, just as in lead poisoning ; a great part of the salt of silver, too, is excreted, as Cloez has shown, even in a short time after the commencement of the treatment, in the urine.

After these purely practical observations, we shall proceed to discuss the more theoretical questions which our subject gives rise to ; and, in the first place, especially as to the essential nature and pathological anatomy of the disease. Duchenne has sketched the disease only from his clinical experience, derived chiefly from private practice ; and, in 1861, he had as yet seen only one dissection of a patient affected with it ; this gave a completely negative result. Both the whole of the brain and the spinal cord appeared (it is true, they were not examined



microscopically) perfectly healthy. Nevertheless, he does not doubt that some change or other, certainly material, must be at the bottom of the disease: and this he is most inclined to seek in the cerebellum or the surrounding parts. The power of co-ordinating the more complex muscular actions, which walking, maintaining the standing position, using the arms, and every voluntary movement require, is, in fact, he says, of a psychical nature, and physiological experiments have long since referred it to the cerebellum. Rolando, in 1809, after numerous experiments, arrived at the conclusion, that the cerebellum was the source and centre of all movements. Flourens, in 1822, somewhat modified this view, as he considered this part of the nervous system to be the seat only of the capacity of co-ordinating all the voluntary movements, while he believed that the latter proceeded from other parts of the central nervous system. A little later, in 1828, Bouillaud still further modified this theory, stating that the cerebellum is the nervous centre only for the regular co-ordinated performance of the several acts of locomotion, and for preserving the equilibrium of the body, while other movements, especially all those connected with speech, are co-ordinated by the anterior lobes of the brain. This view, which he has subsequently adhered to, both in his *Nosography* (1847) and in his numerous lectures, he had arrived at partly from vivisections, in which he irritated, cauterized, or removed portions of the cerebellum—partly from clinical observations, especially of hemorrhage in that organ—and he long since designated the disturbance of the power of motion so produced by the term “ataxy.” The animals operated on were often affected also with amblyopia, or amaurosis, with or without strabismus; and this Bouillaud explained by the fact that the tubercula quadrigemina lie so near the cerebellum that they are liable to be injured along with it, or that, at all events, the irritation is easily propagated from it to them. The same is true, adds Duchenne, of the pedunculi cerebelli, injury of which produces violent pains about the body, and in some cases spasms. Judging from the course of the disease, Duchenne, therefore, believes that it in general begins by attacking the tubercula quadrigemina, the optic nerve, and the motor-nerves of the eyes—that it proceeds thence backwards to some part of the pedunculi cerebelli, and finally reaches the cerebellum. Of the anatomico-pathological lesion of the optic nerve he has, in several cases, been able to satisfy himself ophthalmoscopically during the patient’s life—having thus succeeded in demonstrating the existence of atrophy of the optic disc, which certainly indicates a lesion of the nerve at its origin or in its course. Lesion of the other parts of the brain, on the contrary, as I have already said, he has never yet had the opportunity of observing, and he, therefore, assumes them only by the way of induction. His expectations were, however, soon to be disappointed; as it has since appeared that it is quite a different part of the central nervous system which is affected in this disease. Thus, in April,

1861, Bourdon<sup>a</sup> had the opportunity of making a *post mortem* examination of an ataxic patient, who died of an intercurrent gastric attack. The disease had lasted about six years, but had not, until within the last six months, attained to any great height. The motor anomalies had continued limited to the lower extremities; the upper extremities were completely normal up to the time of death, and sensation was everywhere quite unimpaired. In the left eye there were diverging strabismus, ptosis of the upper eyelid, and considerable dilatation of the pupil; the patient had become myopic, and there was occasional diplopia. The dissection was performed with the assistance of an experienced micrographer, Luys. With the exception of slight congestion in the grey substance, especially in the thalami and corpus striatum, nothing abnormal was met with in either the brain or the cerebellum. Both oculo-motor nerves and, although in a less degree, both abducent nerves were, on the contrary, found reduced to scarcely half their ordinary thickness, were very friable, and were of a grey colour; of the nerve fibres little more than the sheath remained. In the spinal cord, on the other hand, very considerable abnormalities were discovered. All the membranes were, especially inferiorly, highly congested. The posterior columns were, from the tuberculum brachiale, gradually increasing downwards, and most strongly marked in the lumbar region, changed to transparent vitreous filaments—in some places of an amber yellow, in others, of a yellowish red colour. Their consistence was less than normal, but they were not diffuent, nor was their continuity interrupted. This change was due to the nerve fibres themselves; for most of them had, considered as anatomical elements, wholly disappeared, only the empty nerve sheath remaining. In the fibres, of which something yet remained, the cylinder-axis was, indeed, still perceptible; but it was uneven, tomentose, and rather of an amber-yellow. The nerve-tissue was intersected by a great number of capillary vessels. The grey substance had, likewise, from the lumbar region downwards, lost its consistence; many of its nerve-fibres were broken, and of the nerve-cells only some were preserved—most of them were collapsed, ramified at their edges, and covered with many more pigment granulations than normal; they were, in a word, in a state of involution. Deposits of hematosin, evidences of previous congestions, were also met with; all the capillary vessels, too, were here highly loaded. On the contrary, the anterior and lateral columns of the cord were perfectly normal. All the nerve-roots, forming the cauda equina, were flat, ribbon-shaped, greyish or yellowish, transparent, less bulky than normal. This was true particularly of all the posterior roots; and here the microscope also exhibited completely the same changes as in the posterior columns. The anterior roots, on the other hand, were much

<sup>a</sup> Archives générales de médecine. November, 1861, and April, 1862.

less affected; they were essentially only slightly atrophic; the nerve-fibres in them, in particular, were perfectly preserved, with all their elements. Lastly, the ganglia of all the lumbar nerves were altered about in the same manner as the grey substance; they were very red and vascular—their sheath was thickened—their ganglionic cells were in part corrugated, and were abundantly covered with reddish-yellow pigment, though some of them had still retained their normal relation to the surrounding nerve-fibres.

In this case, therefore, a considerable material lesion was demonstrated, not, as Duchenne had been most inclined to assume, in the brain, but in the spinal cord, and there, moreover, not in the anterior, but in the posterior columns and roots, and almost exclusively in these. The question, then, was, whether this lesion could really be regarded as characteristic of this disease, although it was at first so unexpectedly met with, and did not appear capable of giving the key to its explanation, or whether it was only an accidental complication in an ataxic patient; on this point only further experience could justify any opinion. But neither was this wanting. In a short time after the publication of Bourdon's essay, Dumenil, of Rouen,<sup>a</sup> communicated a perfectly corresponding dissection of an ataxic patient. Soon after, Oulmont<sup>b</sup> had the opportunity of examining a similar patient, who, after having perceptibly improved under the use of sulphur baths and various tonics, was attacked by bronchitis, and died in a few days with supervening cerebral symptoms. Even in this case there was in the brain only slight injection of the pia mater. On the other hand, Oulmont, likewise, assisted by Luys, found precisely the same changes in the posterior columns of the spinal cord as were met with in Bourdon's case, except that they were less advanced; in addition, he found the connective tissue of the cord hypertrophied, and a great number of amyloid bodies were seen scattered about in the posterior horn of the grey substance. Bourdon was now convinced, particularly in consequence of the agreement of the results of these dissections with those which he had found mentioned by earlier writers, and which I shall just now describe, that the anatomico-pathological expression for ataxy is a lesion of these parts of the spinal cord. Apart from less essential changes, it may, he says,<sup>c</sup> on the whole, be designated as a degeneration, combined with atrophy, of both the nerve-tubes and the nerve-cells; and he then endeavoured, starting from this point, to reconcile this discovery with our physiological knowledge, and thereby to explain the mechanism of the disease. A few weeks later, Charcot and Vulpian, with more knowledge of the subject and more authority, but on

<sup>a</sup> See *L'Union médicale*, 1862, No. 17.

<sup>b</sup> See *L'Union médicale*, 1862, No. 41.

<sup>c</sup> *Archives générales*, April, 1862.



the self same principle, endeavoured to establish a more elegant theory of the disease. The dissection of their patient, in fact, yielded the same results as the two above mentioned. Here, also, the posterior spinal columns, especially from the tuberculum cervicale downwards, were found to be greyish through their whole thickness, with only here and there slender streaks of intermingled healthy tissue; they were semitransparent and slightly softened. Most of the nerve-tubes had here disappeared; only very few healthy ones were still seen scattered through the midst of the fibrillar stroma, which seemed to be formed of connective tissue, principally, no doubt, the empty nerve-sheaths, but was somewhat hypertrophied; only in the white streaks were the nerve-tubes healthy. Numerous partly ramified congested vessels were met with, their walls everywhere covered partly with fat, partly with granular, partly, lastly, with amyloid bodies; these were present also, in great number, around or in the nervous tissue.

The anterior and lateral columns were perfectly normal; so was the grey substance, except, perhaps, in the apex of the posterior horn, where some amyloid bodies were seen. Of the nerve-roots the anterior were quite healthy; the posterior, on the contrary, were all, from the last cervical nerve downwards, about of the same grey colour as the posterior columns, and were so reduced in volume that they were much slighter than the corresponding anterior roots; in them, too, however, some healthy white streaks were seen. Under the microscope, also, only very few healthy nerve-fibres were found in them; all the others were atrophied, almost as delicate as the normal cerebral fibres, and they had a somewhat varicose appearance; they were exceedingly like nerve-filaments which are formed afresh after a nerve is divided. These changes were all most marked in the dorsal region. The medulla oblongata and brain were perfectly healthy. Both optic nerves were on the contrary, from the optic disc, a little slighter than natural, and were of a greyish colour; and posteriorly, behind the decussation, they became progressively thinner and flatter; not a single healthy nerve-filament was found in them, only connective tissue covered with fat and hematosin. The thalami, corpora striata, corpora quadrigemina, and the pedunculi cerebri were, on the other hand, perfectly healthy.

This result was still clearer and more satisfactory than Bourdon's; the lesion was limited exclusively to the posterior columns and nerve-roots, and to the optic nerves; all the other parts, which in the former case were not wholly unaffected, and the changes of which might, therefore, modify the symptoms, were here perfectly sound; and, both in a clinical and anatomico-pathological point of view, this case may, therefore, be taken as typical.

In addition, two dissections have very recently been contributed; one



was performed by Marrotte, assisted by Luys.<sup>a</sup> The disease was of fifteen years' standing; still the sensibility was wholly uninjured; the mobility, on the contrary, both in the arms, but especially in the legs, was much affected. Acute pleurisy carried off the patient. The posterior columns in the dorsal and lumbar regions of the spinal cord and the posterior nerve-roots were altered precisely in the manner just described; the corresponding anterior parts were healthy. Still the entire of the grey substance in these parts was at the same time somewhat affected; it was of a reddish colour, strongly hyperemic; the nerve-cells were not so distinct as normally, and were covered with abundant pigment. Amyloid bodies were here, too, scattered in great quantity among the normal elements. Not the least abnormality was met with in the sciatic nerve or in the muscles of the lower extremities. The other dissection was performed by Vigla, with Sappey's assistance.<sup>b</sup> The disease had lasted thirteen years; the sensibility was somewhat blunted, but had not become so until within the last few months; the mobility of the legs, but not of the arms, had, on the contrary, been for ten years so much affected that the patient could neither stand nor walk, but had to be always carried about. The eyes were not implicated. The patient died of phthisis. The brain was perfectly healthy. The spinal cord was altered only in its lumbar portion, the posterior columns being here somewhat atrophied and greyish; they were not, however, microscopically examined. The anterior nerve-roots proceeding from this part were quite normal; the posterior roots, on the contrary, were so atrophic that, on comparison with the corresponding roots from another healthy spinal cord, they appeared to have lost from two-thirds to three-fourths of their volume; they were flat, ribbon-shaped, and were of a greyish-red colour. Only a few of their nerve-fibres were still normal; most of them were slender or varicose, had lost a portion of their contents, or in some the latter had in spots, or totally disappeared.

In view of all these concordant results, arrived at by various independent and competent observers, and which, according to a short statement by Bourdon, have been verified also in several cases by Friedreich, of Heidelberg, there can no longer be any doubt that the anatomicopathological foundation of ataxy has been discovered, and that this affection has, according to the general terminology, been transferred from the class of the neuroses, to which it was first referred, to constitute a link in the constantly increasing class of diseases in the central nervous system, where a material lesion is no longer merely suspected, but has been actually demonstrated. There are, indeed, not many who will doubt that the change in the spinal cord and in the posterior nerve-roots

<sup>a</sup> See *L'Union médicale*, 1862, No. 67.

<sup>b</sup> See Trousseau, *l.c.*, p. 826.

is, in the proper sense of the word, the foundation of the disease; that it is the cause of the symptoms, and that it is present from the commencement; in other words, that no really ataxic phenomenon manifests itself before certain parts of the central nervous system have been affected, and that the abnormalities, in their development, constantly keep pace with the progress of the disease. Both processes are in the highest sense chronic; both may for a long time continue stationary; nay, may even retrograde towards recovery. So far as the symptoms are concerned this is sufficiently established by many of the cases on record. As to the nervous lesion, Charcot's and Vulpian's dissection especially is in favour of this view, inasmuch as indications were met with that nerve-filaments were on the point of being reproduced. Placing these two processes in direct relation to one another is so completely justified by our positive knowledge of the relation between the anatomical foundation as cause and the morbid phenomena as effect in all other diseases, and must certainly occur to every one as so natural and rational that it would be superfluous here specially to put forward this view, if such a course had not been definitely called for. This is rendered necessary by the fact that Trousseau, who, from the first, has taken a great interest in this disease, and has accurately traced all its phases, has in this respect put forward an opposite interpretation. He, of course, no longer doubts that the ataxic symptoms and the changes in the nervous system are mutually connected; but he nevertheless continues to refer the ataxy to the neuroses, because he considers the lesion of the spinal cord to be not primary, but secondary, as an effect of a hitherto unknown general condition of the system; and this he assumes as the basis of the disease. How should we otherwise, he says, be able to explain the fact, that parts which are so little connected with one another as the posterior columns of the spinal cord, the motor nerves of the eye, and the optic nerves, should be attacked simultaneously or successively, but always in one and the same manner? In all the pathological development is, in fact, the same. There is from the commencement hyperemia, which subsequently produces atrophy and degeneration. Is it not, then, most natural to assume that the unknown cause producing the ataxy fixes its manifestations, which always remain the same, on different, but mutually connected, parts of the central nervous system; and have we not something analogous to this in the mode in which the other unknown causes, producing hysteria or cachexia exophthalmica, act? Do we not in these diseases also frequently find hyperemia and congestion? Does not the last-named affection in particular often give rise to material changes in the heart, the thyroid gland, and the retina? Seen from an ideal, or, if we will, from an antiquated point of view, there may certainly be some justification for this line of argument, but only on the condition that it be extended much farther than is here indicated. With the same right

by which Trousseau assumes an unknown general cause of the disease in question, he might, it seems to me, assume a similar one in every disease whatever; hepatization of the lung, for example, then becomes only the secondary expression of a general blood disease; and we are not more justified in deducing the pneumonic symptoms from it than in placing the ataxic in connexion with the nervous affections. Trousseau's well-known vitalism here exhibits itself in an extreme degree. In his opposition to organic cysts or to localizers he goes so far that, in spite of the opposition as yet recently raised in the Academy, especially by Bouillaud, he continues to refer cachexia exophthalmica to the neuroses, and now endeavours to do the same with regard to ataxy. It would, in my opinion, be a retrograde movement to follow him in this course; not only for the promotion of science, but also in a purely practical point of view, it is more correct to start from actual data than to seek the mysterious; and, as specially concerns ataxy, its cause appears to me now to be so clear, and so definitely indicated, that I can only wish we had advanced as far with respect to the nature of many other nervous diseases—for example, epilepsy.

It was not until we had the information derived from the dissecting table in our possession that we were fully justified in establishing ataxy as a morbid unity. So long as we had before us only the symptomatological picture our right to do so must always have remained more or less doubtful; the most important phenomenon, the want of co-ordination of movements, might, in fact, according to the opinion of many, for example, of Bourdon, manifest itself also in totally different diseases, for example, in hysteria, chlorosis, poisoning by lead or alcohol; now, since not only all the various symptoms and their sequence, but also their material foundation, have been demonstrated, doubt no longer exists upon the point. But at the same time that we admit this, we must also feel convinced that it is in reality not a new disease which has been discovered and received into nosology; that there can be no comparison in this respect with the discoveries of Bright, Addison, or Virchow, but that we have only a more accurate and more complete development of what we already possessed. To show the correctness of this view, which I have already put forward in the commencement of this article, I may be permitted briefly to quote some cases from earlier writers, which Bourdon in particular has collected.

Cruveilhier has three cases in point. The first is that of a woman, aged fifty-three; the disease was of one year's standing, and manifested itself by violent pains in the lower limbs, loss of sensation in the whole of the lower part of the body, completely normal movements of the legs in the recumbent position, but great tottering and uncertainty during walking; incontinence of urine and feces. On dissection the brain was found to be normal. In the dorsal and lumbar portions of the spinal cord the



posterior columns, but only these, were found changed into a yellowish grey, semi-transparent substance; the anterior and lateral columns, as well as the grey substance, were quite healthy. In the second case the disease was of two years' standing; both the arms, and especially the legs, were affected; the sensibility was much weakened; all the movements, though feeble, were still possible in the horizontal position; the standing position or walking, on the contrary, were quite impossible. Urine and feces passed involuntarily. Dissection exhibited a grey degeneration of the posterior columns of the spinal cord and atrophy of the posterior nerve-roots. In the third case the history states merely that a young amaurotic girl, whose sensibility was unimpaired, but who in point of motion was paraplegic, died of an intercurrent disease. The optic nerves were found atrophied both before and behind the decussation. The posterior columns of the spinal cord were throughout the whole of their length of a greyish-red colour; only here and there were some white filaments met with; all the other parts of the spinal cord, as well as the brain, were quite healthy.

Ollivier gives two cases. One is that of a man, aged forty-eight, whose illness was of thirteen years' standing. It had commenced with pains in the lower extremities; soon after walking became uncertain; he could no longer direct his legs; the one knocked against the other, and in consequence he tottered. Shortly after the arms were similarly attacked. Sensation was much blunted. Vision diminished, and before death he became almost totally blind. Some symptoms of paralysis of the rectum and bladder also existed. The patient died of cystitis, on which peripneumonia had supervened. On dissection the posterior median fissure was found to be of a dark grey colour, and the posterior roots of the nerves of the cauda equina were small, greyish, and atrophied. All the other parts of the central nervous system and the anterior nerve roots were, on the contrary, healthy. The second case is that of a man, aged forty-two, whose illness had commenced five years previously with tingling in the fingers, and had gradually extended first to the upper extremities and so to the trunk and legs; the tingling had then been gradually replaced by incomplete paralysis. All his movements were unsteady and jerking, tottering and slow. His sensibility was blunted. He died of an attack of pneumonia; for the last week there was retention of urine and feces. Quite the same changes were met with in this as in the preceding case, only the degeneration in this instance was more developed above, agreeing with the fact that, contrary to the usual course, the disease had begun in the arms.

Finally, Romberg, too, adduces examples of the same. A provincial physician had, twelve years previously, been attacked with paresis of the legs and with amblyopia; the latter subsequently passed gradually into complete amaurosis, and all the signs of *tabes dorsalis* were



developed; the sensibility of the skin and the temperature nevertheless continued until shortly before death. Romberg and Froriep performed the dissection, and were not a little surprised to find atrophy of the spinal cord to such an extent that the latter had lost one-third of its volume; and also to observe that the atrophy was strictly confined to the lower part of the posterior columns and nerve-roots, both of which had lost almost the whole of their medullary substance, and had become transparent and of a greyish yellow colour. The anterior columns and nerve-roots, on the contrary, were normal. Such pathological changes, he says, are, moreover, not unique, but are frequently met with in *tabes dorsalis*; the changes on which the characters of the disease depend may, in fact, according to his opinion, vary somewhat, but we most frequently find partial atrophy of the spinal cord and of the nerve-roots, especially of the lumbar portion and of the posterior roots. Where amaurosis exists, there is almost always at the same time atrophy of the optic nerves and of the optic commissure, sometimes also of the thalami.

From these examples it is sufficiently clear, that with respect to the nature of this disease, writers have as often as otherwise moved in a circle—just as certainly as it may be assumed to have existed in all ages, has it been observed, followed in its progress, and long since studied in its anatomical relations; but the descriptions given have been forgotten or perhaps unknown; authors have wished to maintain the priority of modern times; and, though sufficient material for its elucidation lay before them in all directions, they have wished, from its symptoms alone, to form a new, hitherto unobserved, independent disease. Now, whether, since the mistake is established, we wish to call the disease ataxy or *tabes dorsalis*, or as Ollivier does, myelitis, is nearly indifferent. Myelitis is certainly the least suitable name, inasmuch as the disease does not present any inflammatory symptom whatever; *tabes dorsalis* has this against it, that it has been employed to designate various morbid conditions in the spinal cord, not a certain, definite, specific lesion; while the name ataxy, on the other hand, is confined to the symptomatology, and does not indicate the material lesion. Least of all ought it to be called Duchenne's disease; Duchenne has in fact only accurately traced and described the gradual development of the several symptoms, and has added but little that is new to the much that was previously known; but he has been in such a hurry to blazon forth his imagined discovery that he has not allowed himself time to await the decision which anatomico-pathological investigations have now brought about, and which must necessarily quite alter his original views.

In conclusion, it remains to investigate the physiological basis of the pathological phenomena. The ophthalmic affections have their palpable cause in the abnormalities in the corresponding nerves of the eyes demonstrated both during life and after death; the pains find their most

satisfactory explanation in the fact that an affection of the central nervous system is present, and the want of intellectual disturbance and the unimpaired maintenance of the voice and speech agree with the observation that this disease has its seat not in the brain, but in the spinal cord. There is, moreover, perfect harmony between the symptoms and the *post mortem* appearances, inasmuch as the former commence in general in the lower extremities and there attain their greatest development, while the latter usually show that the degeneration has commenced, and has made most progress in the lower part of the spinal cord and in the nerves proceeding therefrom; not only the phenomena presented by the extremities, but also the abnormalities of the urino-genital system are thus satisfactorily explained. What, on the contrary, does not appear at first view to be in unison with our physiological experience is, that during the patient's life it is always chiefly the mobility which suffers, while dissection proves that it is exclusively the posterior columns and the posterior nerve-roots of the spinal cord, which are the seat of the degeneration. On reviewing the symptoms, we should, if we suppose it granted that the spinal cord is affected, rather have expected that the localization of the disease should be in the anterior columns and roots, which, on the contrary, are always normal; and if we were, from the *post mortem* appearances alone to draw a conclusion as to the anomalies presented during life, we should infer that sensation only, or at least chiefly, must have been affected; yet the latter invariably suffers much less than the mobility; it is never impaired unless secondarily, and sometimes, as, among others, Romberg's, Bourdon's, and Oulmont's cases prove, it escapes altogether. It is no analogy to the atrophy of the motor columns and roots in progressive paralysis which ataxy presents. Is it then possible to account for this circumstance, to bring the experience and deductions of physiology into agreement with the facts of pathology, or are we compelled with respect to this disease to give up the method which has of late years so largely increased our knowledge in many other directions, and to admit that we must here resign the guidance of physiology? No! for just at the time that the attention of pathologists has been directed to this disease, physiological vivisections have pointed out what was erroneous in many of our previous assumptions as to the functions of the several parts of the nervous system, and have in place of these established others, by the aid of which we are now in a position, to a certain degree, to solve this difficult problem.

After Bell and Magendie distinguished between the two different kinds of nerves, most perfectly represented by the two spinal nerve-roots, it was by all admitted, not only that each of these roots had its particular, definite function, but also that a corresponding relation existed between the different columns of the spinal cord itself. Just as the anterior root was assumed to be exclusively motor, the anterior columns were considered

to be destined exclusively to conduct the impulse of the will from the centre to the periphery; and as the posterior roots were supposed exclusively to convey the sensory impression from the periphery inwards to the spinal cord, the posterior columns were correspondingly believed to conduct it further to the seat of consciousness—the brain. The lateral columns, whence the nervus vagus, the accessory nerve of Willis, and other so-called respiratory nerves spring, were then supposed to rule the involuntary, especially the reflex and respiratory, movements. But all these propositions, which with the exception of the fundamental doctrine of the different function of the nerve-roots, were purely theoretical, and were based upon no practical foundation, have been completely shaken by the results of more modern experiments. That the function of the nerve-roots varies will remain to all ages unassailable; but the properties of these parts have shown themselves to be different from what had hitherto been supposed. The anterior roots are, indeed, exclusively motor; but that they are, therefore, themselves insensible, as was supposed, Bernard's discovery of recurrent sensibility has proved not to be the case. The posterior roots are, indeed, exclusively sensory; but when all influence upon motion has consequently been denied them, more recent experiments have shown that this does not hold good. And as to the spinal cord itself, still more completely altered views have become necessary. This is not the place to take the latter fully into consideration, our present subject renders it necessary only to mention what concerns the posterior columns and roots, and to this therefore our incursion within the precincts of physiology shall be confined.

The anterior root has importance only for motion, and is without any influence whatever upon sensation; after its division the parts to which it is distributed have always in all animals completely retained their sensibility, and are paralysed only with respect to motion. The converse, on the contrary, does not hold good with respect to the posterior roots, as Bernard in particular has pointed out.<sup>a</sup> By experiments, commenced so early as in the year 1842, but only lately concluded, he has proved that the parts whose sensibility has been destroyed in consequence of the division of the posterior roots, can still, indeed, perform all kinds of movements, though not in a perfectly normal manner. To understand the possibility of this state of things, we must remember that each muscle, in addition to nerves of motion, receives also nerves of sensation; these supply the muscles with a peculiar kind of sensibility, which Bell called the sixth or "muscular sense;" while Gerdy, and subsequently Landry, have termed it the "sensation of muscular activity," and Duchenne directly calls it the "muscular sensibility." It makes the

<sup>a</sup> Claude Bernard: *Leçons sur la Physiologie et la Pathologie du Système Nerveux*, T. i. Paris, 1858.

brain, and consequently the individual, acquainted at every given moment with the state of the muscle, whether it be at rest or in contraction, with the degree of the muscular activity, the amount of contraction, &c., and it thus becomes necessary to ensure to the more complex movements the indispensable co-ordination between the different muscles. We must, moreover, remember that every movement may be assumed to be preceded by a sensitive impression, an excitation. For the involuntary movements this probably proceeds from one or other point of the surface of the body, or of the internal organs; for the voluntary movements there is, on the contrary, only one definite inner excitement, and that is the will. It is the expression of a preceding perception, which is preserved as a remembrance; it is therefore only a peculiar form of sensibility, proceeding from the cerebral hemispheres, and which is not conducted through a continuous nerve-fibre to the contracting muscle, but first passes other parts of the spinal cord, certainly ministering to sensibility, before it reaches the motor nerve. This, then, finally sets the muscle in action, and the muscular contraction thus becomes the last link in the series of, and the final expression of a number of connected, but in their nature mutually different phenomena. Lastly, we must remember, that a simple excitation of a single little point of the skin or of the spinal cord does not produce contraction merely of a single muscle, but often gives rise to very complex and extensive movements. With this fact before our eyes, the assumption becomes altogether improbable that the sensory impression is conducted directly to one or more motor nerves, that a direct transition takes place from one kind of nerve to another; it is much more reasonable to assume that in the spinal cord there are many, special, mutually connected centres of motion, whence the motor nerves proceed to the several parts, and which thus determine their co-operation or antagonistic relations; we shall then, in fact, be easily able to understand that by their excitation a complex, but completely co-ordinated movement is produced. Even from these considerations it is clear that all motion is in a certain mode under the dominion of sensation; but this relation becomes still more evident when we study the results of vivisections.

The first fact by which Bernard's attention was directed to the possibility of the importance of the sensibility of a part of the body to its movements—a matter which had hitherto been wholly overlooked—was that when he divided the posterior lumbar nerve-roots on one side of the spinal cord in a frog, the animal bent up only the hind leg, whose sensibility was uninjured, against his fingers, which he held around the body of the frog, while the other hind leg, indeed, moved, but without being directed against any object. A healthy frog always bends up both legs and endeavours with them to push away the fingers and to liberate himself from them. If only sensation had been impaired in the above described operation, such a frog would do the same; but it never does



so. After Bernard's attention was directed to the point he extended his investigations further in the same direction, and soon ascertained the following:—If in a frog so operated on both hind legs be laid out alike on a flat surface, the animal voluntarily draws in only the sound leg again towards the body; the insensible leg it either keeps constantly extended, or, if it makes a movement to draw it back, the limb seems always to go out from the other, still sensitive leg. In leaping or swimming the frog therefore continues apparently to move both hind limbs equally well. If the posterior lumbar nerve-roots are divided on both sides, the same holds good for both hind legs. If they are extended the frog does not draw them back; they are in general not moved at all, unless a still sensitive part of the body—for example, the fore limbs—be irritated; if this be done, the insensible hind limbs, indeed, also take part in the reaction, but their movements are more uncertain than normally; they are convulsive, without object and without harmony. The swimming movements are irregular, and on land the animal creeps with the hind limbs and jumps only with difficulty. If he be held with the fingers around the body he makes no movements to free himself from them. So if the posterior roots be divided in all the four limbs the frog does not move at all spontaneously, but continues to lie immovable; and if he be excited, for example, by pricking the still sensitive head, he makes only unharmonized and irregular movements with the limbs; a combined movement of them, such as, for example, takes place in swimming, is not produced. If, lastly, all the nerves of both fore legs and one hind leg be divided, the other hind-leg being left untouched, we see the latter continue to execute perfectly normal movements; but if we now divide also the posterior roots of this leg without touching the anterior, its movements immediately become uncertain and un-co-ordinated; the animal spontaneously executes not the least movement; even if it be laid on its back in the water, it remains perfectly quiet in this abnormal position, while before, when the posterior roots were not divided, it voluntarily moved this leg with the evident object of turning itself round. All this does not proceed from anesthesia of the skin; for if only the skin be cut off all the four limbs the frog swims about for some time perfectly well; it seems therefore to depend rather on the insensibility of the muscles; and this experiment consequently appears to show that the movements of the muscles, whose sensibility is removed, become un-co-ordinated, because the brain does not acquire the impression of, and the animal therefore does not become conscious of, what takes place in them, just as it seems also to follow that the will cannot dispense with the aid of the sensitive nerves in the production of voluntary movements. Completely corresponding results are obtained too in the mammalia. If, for example, we divide, in a dog, the posterior roots of a limb, the animal, when standing, holds the latter half bent, raised a little above the ground, and

when walking or running he drags it after him ; if he attempts to stand upon the hind legs he falls. If he lies, he keeps this leg constantly extended ; voluntary movements he does not execute with it, while on the other hand, it continues to be the seat of aimless and un-co-ordinated, though powerful movements. But if only one posterior root be left untouched, so as to leave some sensibility in the leg, its movements retain also their regularity, and none of the phenomena just described occur. If, finally, only the cutaneous nerves be divided, even in all the four limbs, there is no impediment to walking or running, and the movements take place regularly, probably because the muscular sensibility is retained. Division of the posterior sensitive roots thus produces in the higher animals also, exclusion of the voluntary movements ; in them, too, therefore, sensibility and the will are physiologically mutually connected. That this obtains also in man, not only analogy, but also daily experience testifies. If, for example, we wish to lift a weight, we first make weak experimental contractions to inform the muscular sense of the strength of the necessary effort ; the information, which thus precedes the final voluntary muscular contractions, is, of course, due to the sensitive nerves.

If it be now shown by these experiments that sensation and motion stand in a much more intimate relation to one another than had before been supposed, it is also easy to understand that results similar to those produced by division of the sensitive nerves may be developed by their atrophy or degeneration. In the latter case we speak, of course, only of a certain degree of change ; by division of a nerve its function is necessarily destroyed, every trace of conduction through it ceases ; in more or less complete degeneration conduction is weakened or ceases in a given number of its fibres, while it still takes place through the non-affected fibres. The parts of the body to which the nerve is distributed will therefore retain some of their sensibility, but their movements will, in a higher or lower degree by their inco-ordination, present evidence of the corresponding lesion. The integrity of the anterior motor roots causes only that the strength of the movements, the degree of contraction, and the energy of each particular muscle are not diminished, but cannot prevent this result with respect to the complex movements.

The information which we can derive, respecting the physiological basis of the disease, by examining the condition of the spinal cord, is indeed also important ; though, on the whole, it is of less value, because the physiological functions of the several parts of the cord cannot yet be said to stand upon so sure a basis as those of the nerves. Many circumstances connected therewith still require to be investigated, and it is probable that posterity will decide many questions in this department otherwise than we have done. Nevertheless we shall see that our present knowledge of ataxy is capable of, to a certain extent, satisfying our requirements, and of explaining the mechanism of the disease.

With respect to the properties of the spinal cord, it is now proved, contrary to Bell's assumption, that the two substances have in it totally opposite relations to what they present in the brain. While in the brain the peripheral grey substance is insensible and the central white matter is sensitive, in the spinal cord, on the contrary, the peripheral white substance is everywhere sensitive, while the central grey substance is throughout insensible. All the three columns of the spinal cord are therefore sensitive; the animal perceives and reacts against any irritation of them; but still there is the essential difference between the sensibility of the three columns, that that of the anterior and of a part of the lateral columns is only recurrent, and is communicated to them through the anterior nerve-roots, while that of the posterior, and perhaps of a part of the lateral columns, is independent. If the anterior column be pricked, the animal perceives and reacts against it only when the part touched is in normal connexion with the nerves arising from it; if, in fact, the corresponding anterior root be divided, this part and the adhering central extremity of the nerve become forthwith insensible, while the peripheral end of the nerve still retains its sensibility; and if the corresponding posterior root be divided, without the anterior being touched, not only the whole of the anterior root, but, at the same time, its place of origin in the anterior column of the spinal cord, loses its sensibility. The sensibility of the posterior column is, on the contrary, not at all affected thereby; it continues, especially towards the median fissure, even if all the posterior nerve-roots be divided. This shows clearly that it is only the posterior columns which are the source and seat of the sensibility, and that it is from them that the latter proceeds to all parts of the body; it proves, at the same time, that the sensibility of the anterior and lateral columns is not communicated to them directly from the posterior columns, but only by a circuitous route, inasmuch as it first passes out through the posterior roots, and then back through the corresponding anterior roots. If all the anterior roots were divided, the anterior columns would, therefore, be quite insensible throughout their whole length; if all the posterior roots were divided the posterior columns would still, on the contrary, everywhere have retained their sensibility. The grey central substance is invariably insensible. If the spinal cord be divided, and the grey substance be irritated in one of the surfaces of section, the animal does not observe it, and does not react against it.

More important still are the recent discoveries respecting the functions of the several parts of the spinal cord, which are now considered to be quite different from what they were believed to be in Bell's time. If his views, described above, were correct, on division of one or more of the columns, the nervous manifestations which he believed to be conducted through them should altogether cease in the parts of the body situated beneath the section. This is, however, far from being the case. The



experiments of Stilling, van Deen, Türck, Brown-Séquard, and many others, have on this point elicited results which quite corroborate one another, and which lead to conclusions totally different from those of Bell. Thus, if in a frog one half of the spinal cord be divided, the subjacent parts of the corresponding half of the body lose neither their sensibility nor their motility; on the contrary, both functions are enhanced thereby. If, as Türck first observed, both hind legs of a frog so operated on be dipped into a very weak solution of sulphuric acid, we shall find that the leg on the side operated on is drawn more rapidly back from the fluid than the sound leg; that it, therefore, feels more acutely, is hyperæsthetic. The retraction of the leg shows that neither has motion been paralysed, as the reflex action is produced; nay, if we look more closely we shall, especially if some time has elapsed since the operation, even find that all the movements of this leg are more energetic, that the contraction of its muscles is, therefore, stronger than that of the sound leg. Hence, quite contrary to expectation, both sensibility and motility increase in the subjacent parts, and this not only in the superficial tissue, the skin, but in all the tissues; even the proper sensibility of the nervous tissue, especially the recurrent sensibility of the anterior roots becomes, as Bernard has shown, below the section greater on the side operated on than on the sound side. The same takes place when the section is complete; if we cut the spinal cord across we shall see that the frog so operated on draws both his hind legs back from the surface of the acidified water more rapidly and with greater force than a sound frog does. Nevertheless there is, of course, this great difference between the effect of these two operations, that only in the first case can the sensation be conducted through the uninjured half of the spinal cord upwards to the brain, and be perceived; in the second case, on the contrary, it must necessarily remain unperceived. With respect to the frog this conclusion is arrived at only *a priori*; in the case of the higher animals it can, on the other hand, be directly demonstrated. Thus, if we perform the same experiment on a rabbit, we obtain precisely the same results, both sensibility and motility being increased. But it is only in the case of the incomplete, unilateral section of the spinal cord that the retraction of the leg is accompanied by a cry, evidence that the animal feels and is conscious of pain. When the section is complete there is no cry; the retraction of the leg is a reflex phenomenon wholly without consciousness. If we now wish to know through which of the columns remaining in the uninjured half of the spinal cord sensation is in the first case conducted to the brain, this may be ascertained by way of exclusion. We then find, in fact, that we may, on this hitherto sound side, not only divide the anterior and lateral columns without preventing the occurrence of a cry, the evidence of the conscious reception of the sensitive impression, but that we may even divide the source of the sensibility itself—the posterior column;



may, that we may go so far as to remove a portion of it. There consequently remains only the grey substance, through which the impression can and must be conducted to the brain; and the result of the whole of this series of experiments, then, is, that the sensibility, which proceeds exclusively from the posterior columns, is conducted, not through them, but only through the in itself insensible, grey, central substance, to the seat of consciousness. The fibres of the posterior roots may, therefore, in part pass over into the posterior columns, but they must at the same time pass, in an essential degree, into the cells of the grey substance, and in this transition lose their original sensitive property. Whether some of them, as many writers—for example, Türck—assume, finally pass over into the lateral columns, is not so certain. The conduction of the sensitive impression to the brain seems, at all events, to take place only through the grey substance.

With respect to the motility, we have already seen above that many arguments may be adduced in favour of the view that the conduction from the brain, which is connected with the voluntary movements, takes place through one or other of the organs of sensation. As to the course of the involuntary reflex movements, our knowledge is more certain, for we know that in their occurrence three stages are to be distinguished:—1, the sensory impression; 2, its transformation into motor excitation; and 3, the movement—that their nervous course consequently proceeds from the sensory nerve, passes through the spinal cord, and ends in the motor nerve. Now, in what part of the spinal cord does the transition from one kind of nerve to the other take place? Most certainly in the grey substance. The sensory nerve filaments pass, in fact, into its posterior part; and we know that from the cells in its anterior horns not only the motor nerve-filaments take their origin, but also that numerous connexions proceed in all directions, not merely to the parts lying above, below, or on the same level in the opposite half of the cord, but likewise to the posterior horns. Each of these cells thus becomes a small nervous centre; and it is, therefore, quite in accordance with this fact that by touching some points of the spinal cord we produce whole groups of movements.

The grey substance must, accordingly, be considered not only as a conductor of sensation, but also as a centre of reflex movements. Where the conduction of the voluntary movements takes place, we, on the contrary, do not yet know with certainty. What, on the other hand, we do at present know with respect to their occurrence concerning the spinal cord is, that the posterior columns have the same importance for them as the posterior roots. Thus, while we have seen that the motility is in a certain manner increased after a simple division of them, a totally different result is obtained when, as Philipeaux and Vulpian were the first to try, the posterior columns of the spinal cord of a dog are divided in two

places, from one to three inches from each other. According to their experiments the sensibility of the hind legs is thus increased, but their motility is much weakened; the animal can no longer stand erect upon its hind legs, and walking is accomplished essentially only with the fore legs, the hinder limbs being dragged after; it is a true traumatic motor paraplegia which is thus produced. Pathological experience confirms these physiological results. In two cases—the one described by Cruveilhier, the other by Stanley—where the posterior columns of the spinal cord were the only parts affected, and in which especially all the nerve-roots were perfectly normal, the sensibility was unimpaired, but the patients could neither stand nor walk, and in the sitting position could only with great effort raise their feet from the ground. Independently of the fibres running in them from the posterior nerve-roots, the nerve-tissue itself of the posterior columns has, therefore, a great influence on the more complex movements, especially on those required for walking or standing. It seems as if the function of these columns is to combine the peculiar actions of the several parts of the spinal cord, and duly to co-ordinate the movements of the individual muscles. If the posterior roots, or merely the fibres passing from them through the posterior columns, are at the same time altered, both sensibility and motility must be blunted, provided that the lesion does not occupy too small an extent; for if its extent be small—if, for example, it occupies only the space of one or two vertebræ—it may, as pathological experience has shown, exist in man without being recognisable by any symptom.

Lastly, we must remember that sensibility presents itself under many different forms, of which the most important are—1, the general sensation of contact; 2, the sensation of pain; 3, the sensation of temperature; and 4, the perception of muscular activity. That each of these forms may by itself be altered without the others suffering thereby, or, even if they are affected, suffering in the same manner; and, lastly, that one and the same influence, but of different strength, or one and the same organic lesion, but at different periods, may gradually induce their exaltation, perversion, and diminution, or extinction. In these respects the experiments with compression of the nerves, instituted by Bastien and Vulpian, are especially interesting. Thus if we compress one or other nerve—for example, the ischiatic nerve—the general cutaneous sensibility is soon blunted; but if we at this moment prick or pinch the skin, or touch it with a cold body, we cause very violent pain; and it is only upon stronger compression that this hyperesthesia is replaced by anesthesia. The same holds good with respect to the spinal cord; pathological observations as to the effect of compressing exudations or of tumours sufficiently establish this point. But if this be the case, it is not difficult to understand that the disease under our consideration, which is due precisely to congestion of the posterior columns and posterior nerve-roots of the

spinal cord, and to the consequences of the same, may be attended with excessive pain.

We shall now, in conclusion, find that we shall be able, at least in a certain degree, to refer the symptoms of ataxy to this physiological basis. The cause of the affections of the eyes has already been stated, and is sufficiently evident; the pains are explained, as has just now been mentioned, by the congestive process and its results; the disturbances of mobility and of sensation find their adequate cause in the atrophy and degeneration of the posterior columns and roots, and in the close connexion which modern researches have proved to exist between sensation and motion; and lastly, the abnormities in the urino-genital system depend upon the numerous anastomoses of the cerebro-spinal nerves with the sympathetic. The symptom which must still be looked upon as most enigmatical, is properly the state of the sensibility. Sensation certainly would necessarily be wholly extinguished so soon as the posterior roots were entirely destroyed, and accordingly we have, undoubtedly, in the fact that in none of the dissections on record such complete destruction was met with, but that some nerve fibres were still always preserved—an explanation of the circumstance, that in most cases the sensibility was only more or less weakened, and not entirely removed, and especially that it was not equally affected in its different manifestations; but beside these cases there are others, as I have already several times stated, fewer in number, but entirely trustworthy, where the sensibility was not found to be in any way affected, and where, nevertheless, the disorganization of the posterior roots was very far advanced. Are the nerve fibres still remaining sound which are met with in the midst of the destroyed ones, sufficient to maintain the function of the nerve without any discoverable change? Or are we, as, for example, Charcot and Vulpian do, justified, in the face of so striking an opposition between the results of clinical experience and some of the most certain conclusions of experimental physiology, in consigning the former to oblivion and in inferring that some mistake or other lies at the bottom of the statements of the several observers with regard to the normal existence of sensibility in each respect? Are we, for example, warranted in believing, as Jaccoud first pointed out, that the muscular sensation, like that of the skin, has different manifestations—that each of these may be separately affected, and that we therefore ought not to assume it to be decidedly normal, because the patient, for example, has the sensation of contraction of the muscles, and feels pain in them when they are pressed upon, or when they are the seat of convulsive spasms? This question we must leave unanswered, there is a contradiction involved in the point which we are not yet in a position to get rid of. The same is the case when the state of the sensibility is considered from quite another aspect. Thus, if we reflect, that more or less of the fibres of the nerve-roots are destroyed, we should,

in the patients in whom a greater or less degree of anesthesia of the skin exists, expect that this would be of such a nature that parts would be found with healthy cutaneous sensibility scattered among others completely anesthetic. This, however, is never the case; the sensation is uniformly weakened over the whole of the affected surface of the skin; it is no where normal, but neither is it anywhere entirely extinguished. This phenomenon cannot be explained in any other mode than by taking refuge in the view adopted by most modern physiologists, that the sensibility, even in the state of health, radiates, that is, that it is found not exclusively where distinct nerve-fibres can be demonstrated, but that it has a greater extent. Its existence is therefore not, in the strictest sense, connected with the immediate presence of nerves; the space between two nerve-fibres without lateral branches will, in fact, on this assumption, also be sensitive. Lastly, neither does our present knowledge teach us what the reason is that sensation in general, even in extensive ataxic lesions, is not more affected than is the case; while, on the contrary, we see that, in other apparently less intense lesions, both of the spinal cord, and of the brain, for example, in apoplexy, all sensation, both direct and indirect, is completely and permanently lost.

Another circumstance which, in like manner, cannot yet be explained, is the connexion which in this disease exists between the spinal lesion and the affections of the optic nerves without the intervening nervous parts being in any way implicated. We have no indication on which to base any opinion as to the cause of this fact, and yet it is not possible to consider the abnormalities as independent of each other and only accidentally co-incident; we are, on the contrary, compelled to regard them as two connected joints in the chain, between which the future may reveal the uniting link.

One short remark still remains to be made before closing this article. The question must, in fact, suggest itself to every one, whether the motor disturbances are merely the result of the weakened sensibility, or whether they exist more independently. From all that has gone before we must adopt the latter view. There can, of course, be no doubt that the impairment of sensation, whether it occurs in the skin or in the muscles, or in both, must contribute considerably to increase them, especially when vision is at the same time affected; but the cases in which no anomaly of sensibility is demonstrable are decidedly against assuming its disturbance to be the primary affection. Before these cases were observed, the decision was not so easy; both opinions had their advocates. Thus Duchenne shortly after his first description of the disease as a new morbid affection, was violently opposed by Landry, who had in 1855 communicated a series of observations respecting patients with paralysis of the muscular sensibility, and now asserted that ataxy was only another name for this disease, that the priority of this discovery belonged to him,



and that Duchenne had consequently been guilty of a literary mistake. Each party soon acquired adherents; Trousseau took Duchenne's side, and asserted that in ataxy, besides the symptoms met with in the paralysis described by Landry there are others, and that the differential diagnosis is therefore possible between these two diseases; Landry's opinion, on the other hand, was supported by Becquerel, Monneret, Marcé, and others. After the publication of Bourdon's and other similar observations this contest was given up; it appears to me, too, that the question must be looked upon as having been thereby decided. On this point Duchenne has had the right on his side.

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## CLINICAL RECORDS.

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*Cases Illustrative of the Loss of the Proper Use of Letters, and of Words, as a Result of Cerebral Disease.* By WILLIAM DANIEL MOORE, M.D.. Dub., M.R.I.A., Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

The following cases I should gladly have offered to my friend, Professor Banks, as a contribution towards his important memoir "On the Loss of Language in Cerebral Disease," which appeared in the 77th number of this Journal (for February, 1865), had I been aware that such a paper was in preparation by him. As I lost the opportunity of doing so, I would now place them on record, as an appendix of illustrative cases to his interesting essay. In doing so, I regret very much that my notes of the first case I shall mention are very imperfect; and that of the second case, which occurred to me some years previously, I have no written record whatever. Nevertheless, as the particular point to which I wish to call attention—the loss of the proper use of letters and of words, as a result of cerebral disease—is in one instance noted, and in the other is, by a couple of examples, indelibly impressed upon my memory, I trust I may be allowed, even now, to place upon record the instances to which I have alluded, as illustrations of the ever-varying phases of the symptoms of brain disease.

CASE I.—On the morning of Sunday, December 10th, 1854, I was called to see E. H., Esq., aged fifty-six, who had all his life enjoyed good health, except that latterly he had been annoyed with dyspeptic

symptoms, and especially with uneasiness in the left hypochondrium. I found him, at six o'clock, a.m., apparently suffering from colic, which had set in about five hours previously, and for which he had taken castor oil, and attempted to administer to himself an injection. A large emollient enema, thrown up with the long tube, brought away a copious accumulation of fecal matter mixed with scybala. This relieved the patient for a short time, but in the course of an hour or two he was seized with intense pain and tenderness in the left hypochondrium, accompanied with much uneasiness in the entire of the transverse arch of the colon; his tongue was dry, and brownish in the centre. Sir Henry Marsh then saw him with me. Two relays of eleven leeches each were applied to the seat of tenderness, and calomel and opium were freely given; this treatment was attended with much relief; poppy-stupes also gave great ease. On the 12th the patient referred the pain more to his back, and he once complained of uneasiness in one testicle. Mr. H. progressed favourably until the 16th (his pulse had never risen beyond 92), when, with great languor and lassitude, he passed a quantity of bloody urine, acid, s.g. 1.016, free from sugar, containing a fair amount of urea, depositing much mucus, numerous blood-corpuscles, some epithelium, and tubular casts. Next day the blood was much less in quantity; on the 18th it had ceased to be perceptible to the naked eye, but blood-corpuscles were still visible in considerable number under the microscope. The urine now also deposited numerous large crystals of uric acid, many of them agglomerated, and some arranged in long series, having apparently formed on hairs. 19th.—Mr. H. seemed low and weak, but was otherwise going on well. It was a question whether the pain Mr. H. suffered from, as well as the hematuria, might not have proceeded from the presence of a renal calculus. Some apprehension of incipient Bright's disease was also entertained.

In October, 1858, Mr. H., who was then at his residence in the west of Ireland, on returning from church, fell asleep, and awoke hemiplegic. He was largely bled and actively treated. The loss of power soon passed off, but his sight continued impaired; and his memory, especially for names of places and persons, was very much affected.

Nov. 11.—To-day I observed that when Mr. H. (who had come up to Dublin for further advice) closed his left or sound eye, he could not open the right or affected eye. He appeared not to see with the latter, for when it was held open, the left eye being shut, the right eye did not take the direction of the object at which the patient was desired to look. When the right eye was closed, the left remained open, and Mr. H. could see. When both eyes were open, no peculiarity was observed, but the inability to raise the lid of the right eye when the left was shut, and *then only*, was remarkable.

November 18, 1858.—The above-described phenomenon was not

permanent—neither Sir Henry Marsh nor I succeeded in observing it two or three days later. But this day I found that, though Mr. H. gives right names to persons and things, and speaks correctly—except that he is sometimes at a loss for a word—he completely miscalls letters, calling A K, &c.

Feb. 16, 1859.—Mr. H.'s general state is one of improvement; he has been out several times, *but, notwithstanding that he is able to draw and sign a cheque correctly, he still miscalls letters, and consequently cannot read a word.* He complains of shooting pains in his right side, and though there is no apparent loss of power, that side is evidently wasting. As to letters, a slight improvement was apparent the other day, when he spelled and read correctly "the ne," though he miscalled the next letter, which was "w."

Mr. H. died of a sudden attack of apoplexy October 1, 1860. He had not experienced much improvement since the last report. He was seen by Sir Henry Marsh, who that day two months, December 1, 1860, himself died suddenly of a similar attack.

CASE II.—On Friday, the 4th September, 1840, R. H. G., Esq., aged about fifty-six, was attacked, at his seat in the County Wicklow, with paralysis of one side of his face. Being alarmed, he came into Dublin, and in the course of the day he became completely hemiplegic. Sir Philip Crampton and Dr. Stokes both saw him with me. On Thursday, the 10th, he was much better; and, sitting up in his room, said to me—"I will go to bed to-morrow." This expression he repeated several times; he meant that he would go home the next day. On the following day, however, he got a much worse attack of hemiplegia, and now the loss of connexion between ideas and words was complete. On one occasion I was much puzzled by his saying to me repeatedly, "Clean my boots." Finding that he was not understood, he became much excited, and cried out vehemently—"Clean my boots by walking on them!" At length it was ascertained that the cause of his disquietude was the shining of the candle on his face, and that the object of his unintelligible sentences was to have the curtain drawn; when this was done he appeared quite gratified. Mr. G. slowly improved from this second attack, but became a lunatic, in which state he survived for fifteen or sixteen years.

In the latter case it is remarkable, that although, after the second attack, there was no connexion whatever between the words used by the patient and the ideas intended to be conveyed, Mr. G. formed complete sentences.

*Cases treated at the Dispensary for Skin Diseases, Bishop-street.* By T. W. BELCHER, M.D. Dubl.: Fell. R. Coll. Phys. &c. (No. III.)

In the February and May numbers of this Journal I gave particulars of several cases illustrative of important points in practice. I now

venture to give one illustrative of the great difficulty of diagnosis, and classification, in many examples of cutaneous disease, as well as the variety of treatment, and the amount of patience often necessary to effect a cure.

CASE XI.—S. P., aged fifty-six, a tradesman's wife, who was in the habit of washing clothes a good deal, was admitted 7th October, 1864, with an eruption on her hands and arms, and a slight erythema on one cheek. She has had this affection to a greater or less degree for some years, and has tried many remedies under the advice of various physicians, with none of whom she had attended sufficiently long to get any good. The eruption is symmetrical on both forearms and hands; in shape nearly circular, attended with intense heat and itching, loss of appetite, and general *malaise*. The disease is in three different stages: 1, on the right arm, a vesicular eruption, with what is commonly known as an eczematous discharge; 2, on the left arm, an impetiginous eruption, with purulent discharge; and 3, on the left cheek, a circular erythematous blush. According to Neligan's classification, the right arm would be said to be affected with eczema impetiginodes; the left with impetigo figurata; and the face with erythema simplex. To this I may add, that in about a fortnight after admission the diseased skin of the left arm became *rimous*, answering to the "eczema rimosum" of Dr. M'Call Anderson (*Practical Treatise upon Eczema*, p. 23).

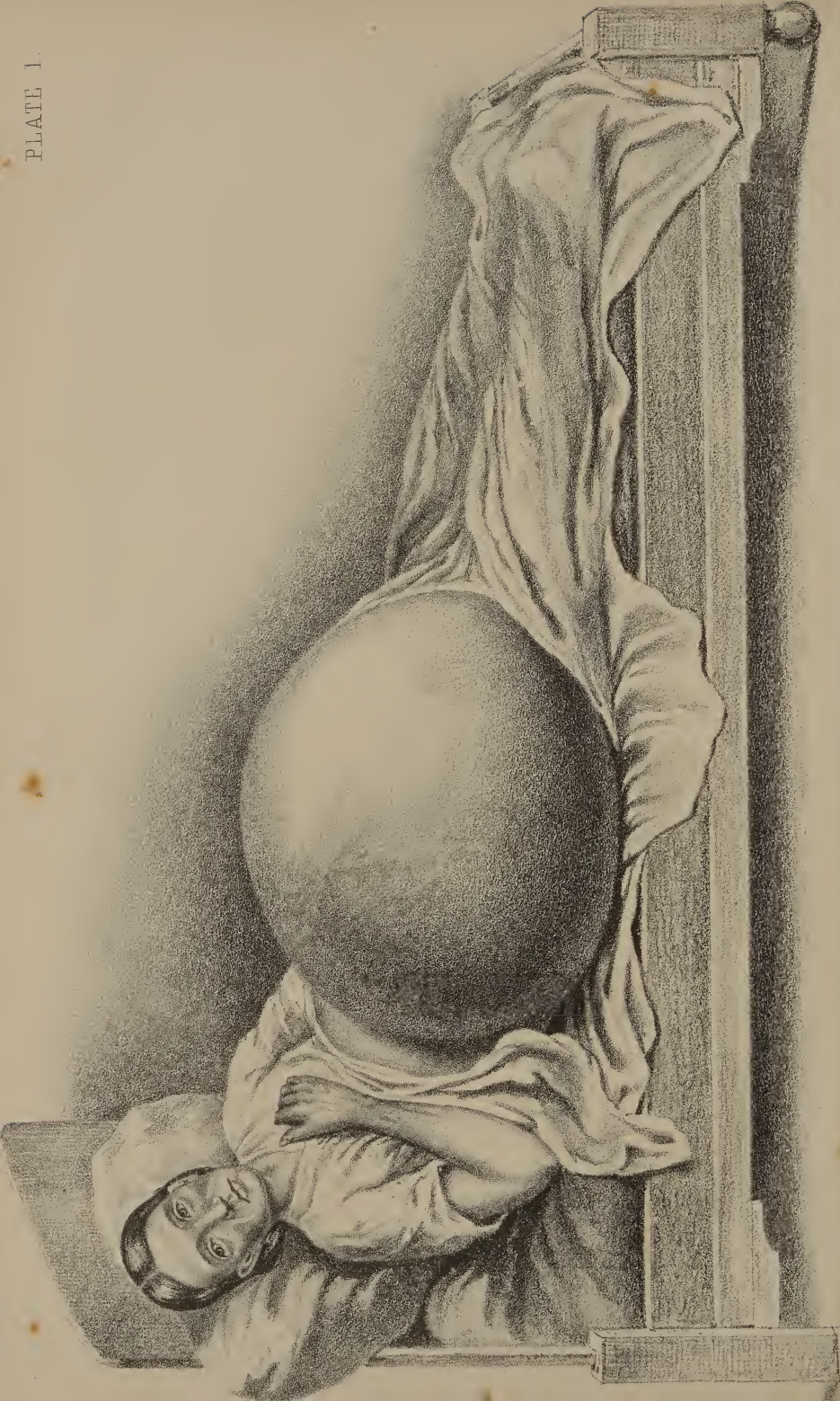
In the first instance, she got three minims of liquor arsenicalis, thrice daily, after meals; and she was directed to keep the affected parts covered with mucilage of starch, to be changed daily. Her system being soon under the influence of arsenic, its use was suspended, and quinine substituted for it. After a few days the use of the former mixture was resumed, with directions to take a small quantity of sulphate of magnesia twice weekly. Although the disease appeared to get worse shortly after beginning the use of arsenic, in about twenty-one days she seemed to get very much better. The arsenic disagreeing with her a second time, quinine was again substituted for it; and the mucilage of starch failing to do any good, she applied to the skin linseed poultices moistened with liquor plumbi dilutus, and used the following ointment:—℞ Ferri sulph. sicc. gr. iii.—Chloroformi M. v. Ung. simpl. ℥i. M. This relieved the local symptoms very much, and the chloroform was afterwards increased or lessened in quantity as occasion required. Again she took to the arsenic, and now with better effect. Since then she was able to take it for some time, until the peculiar arsenical desquamation, described by Hunt, appeared. She was again compelled to stop the arsenic, and take to the quinine; the eruption, however, gets better, and the itching and exudation alternate from one arm to the other. About two months after admission she commenced taking quinine in combination with arsenic. This served



her very much. She also applied Hebra's tincture (described in the May number of this Journal, p. 496), to the skin, but this did not answer, so tincture of iodine, prepared as described in the February number of this Journal (p. 253), was applied to the surface for a considerable time with good result. Chloroform was now the only external application which relieved the itching; and it was freely used with this view. The eruption gradually narrowed in its circumference; the nature of the discharge frequently alternated; now the disease seemed almost gone, and again it appeared to gain fresh vigour. At last it entirely disappeared; and after she had taken the combination of quinine and arsenic for some time subsequent to the apparent cure, she was discharged on the 19th of May, 1865.

If I understand Mr. Erasmus Wilson aright, it would seem that this case is what Willan would have called "washerwoman's itch;" and what he (Mr. E. W.) calls "eczema dorsi manus," "commonly termed psoriasis" (*Student's Book of Cutaneous Medicine*, 1864, p. 87). The case just detailed shows how necessary patience and variety of treatment is in many instances of cutaneous disease; while I confess that I cannot properly classify it according to any system, unless I accept Dr. McCall Anderson's statement, "that cases of eczema are often met with in which an erythematous state of the skin, vesicles, pustules, papules, and fissures are met with in a combined form."—(*Op. cit.* p. 3). This is one of several doctrines held by him in connexion with Hebra's theory—that eczema is a disease of wide comprehension, and of great variety; and that (contrary to the dogma of Willan) it is not exclusively vesicular.





Mr. Butcher on Ovariectomy and the after treatment of the patient

THE  
DUBLIN QUARTERLY JOURNAL  
OF  
MEDICAL SCIENCE.

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NOVEMBER 1, 1865.

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PART I.  
ORIGINAL COMMUNICATIONS.

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ART. X.—*On Ovariectomy, and the After-treatment of the Patient. Illustrated by Cases Successfully Cured.* By RICHARD G. BUTCHER, M.R.I.A.; Honorary M.D. of the University of Dublin; Vice-President of the Royal College of Surgeons in Ireland; Fellow, Licentiate, and Member of Council of that Body; Surgeon to Mercer's Hospital, and Lecturer on Clinical Surgery; late Chairman of the Surgical Court of Examiners.

CASE 1.—ENORMOUS BILOCULAR OVARIAN TUMOUR; OPERATION BY EXCISION; PERFECT RECOVERY.

CASE 2.—ENORMOUS UNILOCULAR OVARIAN TUMOUR; OPERATION BY EXCISION; PERFECT RECOVERY.

AMONGST the capital operations which have been rescued from obloquy within the last few years there is not, probably, one of greater moment, of deeper interest, than that of ovariectomy. By the painstaking investigation so strongly stamped upon the pursuit of every profession in this nineteenth century, difficulties that were too hastily put aside in former days, have been brought out again, unravelled, rescued, and overcome; the bold proceeding under



consideration affords, I conceive, as good an illustration of the fact as could be adduced. The operation of ovariectomy, when judiciously applied, in the present day offers as fair a chance of success as most other capital operations of magnitude, provided a judicious selection of the cases be made; and there is no excuse now for gross errors being committed in diagnosis; there are ample sources of knowledge from which information may be obtained to direct the practitioner and to secure the safety of the patient. I regard the operation with pride as an offspring of British genius, cultivated by British industry, and, above all, as being beneficial to mankind. First suggested by William Hunter—performed in Scotland, in 1823, by Lizars—attempted by Grenville, in London, in 1827—and successfully performed, in 1836, by Mr. Jefferson, of Framlingham. Until 1858 ovariectomy was at a very low ebb in London, when some active spirits took up the subject in a true practical light; and here the name of Mr. Spencer Wells must ever stand most prominently forward as associated with the restoration of the operation to its legitimate place, now recognized in operative surgery; and the manner in which he has handled the subject secures for him, likewise, the enviable position of being considered a scientific observer, a bold operator, and a most reliable authority. I fear greatly, however, there is a recklessness (and it is deeply to be deplored) about the selection of the cases operated upon, as if rather a panting after the achievement dulled and silenced the warnings of better judgment; by some it may be said I speak too plainly; but the statistics of recorded cases bear me out in this warning appeal, be it ever so unpalatable. I have watched with anxious care the progress of this operation, and with exultation the steady, good, and truthful reports given us by some, whilst by others the mixed-up cases, heterogenous, bringing with them nothing but uncertainty, and only affording evidence either of negligence in examination, ignorance in diagnosis, or rashness in proceeding. I would sum up my views on the matter in this way:—

A brilliant, rapid operation is very important, greatly to be admired, and most commendable; but the cautious selection of the case—the careful, judicious after-treatment—the attention to minor details, &c., &c., are never to be forgotten; these alone secure *success*, and *that result* should alone confer distinction or honour upon the operator.

I shall now detail two cases (the only instances in which I

performed ovariectomy) illustrative of my views upon the mode of operation, after-management, &c., and lay down a few deductions relative to the most important and disputed points in connexion with this great operation.

CASE I.—*Enormous Bilocular Ovarian Tumour; Operation by Excision; Perfect Recovery.*—Jane Goodwin, aged twenty-four years, a milliner by occupation, single, admitted to Mercer's Hospital March, 1865, with ovarian tumour of enormous size. She stated that up to three years and a-half before her admission her general health was excellent; the catamenia were regular, having first appeared when she was fifteen years of age. As to the commencement of the disease which now oppressed her, she first perceived a slight swelling at the lower part of the abdomen, but could not accurately localize it more to the left than the right side; there was at this early indication an absence of pain, yet usually present a sense of constriction or tightness, from the pressure of her clothes, as if fastened too closely; still it is remarkable that this sensation would disappear during the period of her monthly illness, while on its subsidence the feeling of distension would return in a mitigated form, or its arrest be entirely restored. The swelling gradually increased in about four months after these symptoms were first complained of, and then began gradually to assume a definite form, and about the size of a small melon, somewhat more oblong in its oblique and vertical axes, and situated to the right of the umbilicus in its prominent part; otherwise it might be said to hold a mesial position. The patient at this time, when in the erect posture, on looking down upon the abdomen could clearly discriminate the increased prominence of its walls in projection about three inches, and to such an extent as to make the space between the left iliac fossa and umbilicus quite depressed. On the patient handling the tumour at this time she could press it downwards and somewhat to the left side; into the same locality it would also descend or fall when the patient lay upon her left side either for repose or sleep; she avoided lying on the right side as the tumour's weight was more oppressively felt, and if she endeavoured to maintain this position for more than a few minutes pain quickly supervened, as if some force was dragging down the parts from within her left side.

During the incipient growth of the tumour she constantly felt a burning disagreeable sensation throughout the abdomen, but never acute pain; neither was there nausea or sickness present when the

stomach was empty; but occasionally, after eating, a loathing and disgust even at the idea of a repetition of the meal; occasionally she suffered from weakness, palpitation of the heart, and general uneasiness in its region. When suffering for about eight months she was fully as large as a pregnant woman at the ninth month; and, from the gradual growth and increase of size, and attendant symptoms, in fact she was pronounced to be in that condition; however, on more careful examination the true nature of the affection was arrived at. Shortly after this the constitutional symptoms assumed a far more severe type—from the weight and dragging of the tumour the greatest fatigue and lassitude were occasioned; from the compression of the abdominal viscera great disquietude, apprehension of suffocation, sickness, and vomiting were occasioned; the normal action of the intestinal tube was dulled and arrested, so that obstinate constipation was present, lasting frequently for a fortnight, and then only relieved by active and drastic purgatives; the secretion of urine was small, micturition painful, and accomplished with great difficulty. She was compelled to remain in bed for the three months previous to her admission to hospital, owing to an indescribable sensation of weakness, as if she was dying, whenever she attempted to assume the upright position, or make the slightest effort to walk. On the date mentioned she was admitted to hospital, and a more careworn, dying, wretched-looking creature was never the recipient of its mercy. This young woman (only twenty-four) was haggard, thinned, bearing on her emaciated features all the traces of her past and present suffering. The face was livid, the lips purple, from congested circulation; the absorption of every particle of fat about the face impressed upon it the pinched and wrinkled stamp of advanced age; and from the same cause the eyes were lax and ill-supported in their sockets, sunken, while their expression, brilliancy, and power were more effective, dazzling, and unnatural; there is an expression of eye in this affection which I have recognized often, and which I have not observed in other diseases. The emaciation of the whole body, except the abdomen, was extreme; the face, the thorax, the back, the extremities, were reduced to mere skin and bone; yet the abdominal walls on the lateral regions were thick, well covered with fat, while in front the deposit was far more limited, and more particularly so from a little above the umbilicus downwards to the pubis; indeed here the tumour seemed to be most prominent in a very marked way. The measurements of the body, as will presently be seen, were

enormous—distinctive evidence accounting for the oppressed breathing, the oft-times-felt strangling suffocation, the unwearied restlessness to obtain relief from position. The measurements of the body were very accurately taken; the girth at the umbilical level was four feet eleven inches, from the ensiform cartilage to the umbilicus seventeen inches, and from the umbilicus to the symphysis seven inches; from the right anterior superior spine of the ilium to the umbilicus fifteen inches, and from the left anterior inferior spine of the ilium to the umbilicus fourteen inches three-quarters.—(See Plate I., taken from a photograph before operation, and which gives an admirable likeness of the patient.) On balancing the abdomen from side to side between the hands the entire moved in one solid mass; by percussion of the tumour in front, above and below, it was dull; by percussion of the tumour on either side, two-thirds of its lateral walls from before backwards, were dull, while the posterior third was clear, tympanitic—clear indication of the course of the intestines in their normal site; no hard or solid matter could be detected as laid down in this abnormal deposit. Palpation of the tumour from side to side gave evidence of fluid—fluid undulating with but little intervening partitions; succussion of the tumour gave the undulating shock characteristic of the fluid contents; this conclusion must be taken with the limitation assigned; it was a succussion produced rather by the rapid movement of the tumour in its peritoneal envelopes than of the fluid within the strained, tightened ovarian cysts; yet to the educated hand each is divisible, limited, and clear; the entire tumour seemed movable, painless on general motion, but acutely sensitive if leant upon in centre, when tendency to vomiting was rapid, uncontrollable, and produced; pain, too, was deeply complained of by pressure exerted over the left iliac region.

This young woman was a virgin, and on examination per vaginam the uterus was movable; the walls of the abdomen, though greatly strained, yet were traced by numerous large venous trunks, loaded by anastomosing capillaries, and large emunctories in their course upwards towards the heart—no doubt the blood from the deeper parts, owing to mechanical obstacles within; retarding it in its onward flow; thus driven to the surface it enlarged this compensating chain of vessels scattered superficially, and so enlarged, distended gave characteristic specialty to the vascular distribution.

On the patient's admission to hospital every means possible was had recourse to, to try and restore some quiet, some repose of body,



and mind; in spite of every effort, every means, all precaution, the occasional vomiting came on; the nausea, and depression, and loathing of food was persistent; the rapid, fluttering, irregular pulse was marked, with occasional tendency to weakness and to faint; the terror at approaching drowsiness or sleep, lest death should follow—the pain, the weariness, and fatigue of constrained position—the occasional cramps and terrible pain from constipated bowels, protrusion and constriction of the gut, and the agony created from inability to empty an over-distended bladder, combined to create such an aggregate of suffering that she strenuously urged the execution of any operation which would afford even a remote chance of relief from the most excruciating combination of morbid circumstances. After careful consideration of the case in all its bearings, in conjunction with my colleagues, I came to the conclusion that it was one which afforded every chance of recovery by operation; and on the morning of the 15th of April I operated after the following manner:—

The patient, previous to her being removed from the ward, was clothed in flannels—drawers, jacket, stockings; the vesical contents were drawn off, and a large sheet of waterproof cloth, with an oval hole cut in it, and so adapted to the prominent abdomen as to leave exposed its forepart and sides; for two inches deep around the margin of the aperture adhesive plaster was laid on, so that the entire circumference of the opening lay strictly adherent to the skin, and by this arrangement the flannel clothing was protected from ascitic fluid or blood. The patient was then brought into the theatre of the hospital, and placed upon the ordinary operating table; she was so arranged that the legs could hang over the end of the table and the feet rest upon a shelf or support, elevated to the required height; the body being enormously tense, distended, and the respiration a good deal embarrassed, not only from the ascent of the diaphragm by pressure from below, but likewise from the impeded functions of the intercostal muscles, owing to their being put upon the stretch and maintained so; while the lower set of muscles those in connexion with the false ribs were almost paralyzed from the strain put upon them by the eversion, almost dislocation, of their cartilaginous extremities. The patient then was allowed to assume as elevated a position for the head, neck, and chest, not only as consonant with what was required to be done, but primarily as securing a free respiration, at least as unembarrassed as it could possibly be secured in the recumbent position. Chloroform was

then administered, and the patient in a short time rendered insensible. Standing in front of the patient, and on the right side, I commenced an incision at a point about an inch below the umbilicus, in the centre of the linea alba, and carried it directly downwards for four inches; the integuments, the fat, the superficial fascia were all divided by this sweep of the knife, from the point where the knife was first laid on, to that from which it was lifted off, the linea alba was laid perfectly bare; from this extensive gash the bleeding was but feeble, requiring no special care. The next step in the operation was to divide the linea alba; to effect this in the most satisfactory manner a slight vertical stroke of the knife was made at the junction of the two upper thirds and the lower third of the part exposed, because most bulging and prominent; the wound was not larger than that which would admit an ordinary director; this instrument being passed first upwards beneath the fibrous membrane, and resting upon the peritoneum, the strained bands were divided to the very upper angle of the wound; the director was then turned downwards, and the linea alba slit freely upon it to the lowest part of the angle of the external wound; the peritoneal sac presented itself next throughout the long trajet of the wound; I had written "*projected* itself," but erased the word, for it was not so; the cellular layer exposed of the serous membrane, lay, as it were, collapsed upon the sac, and required to be pinched up with the forceps; and so, to be safely divided, a small aperture being made in the lifted portion by the knife placed horizontally—just as a hernial sac should be opened—a director was insinuated, and the serous membrane slit upwards and downwards throughout the entire track of the external wound. A small quantity of serous fluid gushed out, and now the whole sac of the ovarian tumour, convex, prominent, blocked the gaping wound. A large trocar projecting from its canula was thrust, canula and all, into the tumour; when up to the hilt a retractive spring withdrew the trocar, and quickly the fluid followed this part of the instrument, until it escaped by an aperture in the canula thus uncovered as the trocar glided out; in connexion with the opening an elastic rubber tube was attached, which conducted the fluid into a bucket beneath the table. As the fluid flowed away a wide flannel tailed bandage was made to encircle the lower ribs, epigastrium, and hypochondriac regions, with gentle, increased, and moderately persistent pressure, so as to compensate for that enforced upon the deep vessels, but now gradually being taken away. As the fluid flowed

out, and as the pressure was steadily made from above downwards, so the sac became more relaxed in the wound; it was grasped by a vulsellum, and gentle traction made upon it in addition to the forces already effective in favouring its delivery; as I gently drew it out from its berth the projecting proportions were sustained in the hands of an assistant; by these gentle means much had come out, but adhesions sufficiently strong required the introduction of my hand to break them down; with care, gentleness, and precision this was accomplished; and now the entire tumour being empty projected from the wound.

The pedicle of the tumour was dealt with in the following way:— It was held up to the light, the most transparent part selected, and a double ligature passed through it; each half of the pedicle was firmly, securely tied with a strong silk ligature and doubly knotted; the double ends of each were left undivided; a very large artery, the size of the radial, was ligatured separately; one ligature was placed again upon the pedicle, about three-quarters of an inch away from the others, a simple ligature around the entire pedicle being firmly secured; then a hot wedge-shaped cautery was applied to the pedicle, between these ligatures, and by a sawing movement was made to sever the substance; and so the tumour was liberated, cast off; blood seemed to flow from the cauterized extremity of the pedicle left behind; the application of a flat cautery to the surface would not stop it; the bleeding was checked by dissecting out from the pedicle a very large artery which was ligatured; the pedicle was then transfixed with a sharp-pointed needle carrying a strong double silk ligature, and each half tied separately with considerable force; the ligature first put on was snipped across, and the pedicle cut away to within a quarter of an inch of the part where the double ligature was applied; the large artery, with its ligature, hung free from the rest; the ligatures were next brought out of the wound and carefully protected. Not a drop of blood got into the abdominal cavity; therefore no sponging or stopping. The lips of this deep wound were then brought together by four points of interrupted suture; a long needle, slightly curved at the point, readily passed the strong silk cord through peritoneum and all structures to the surface, and so the four were inserted; the ligatures of the pedicle and large artery within it were brought out about the centre of the wound, so that two of the deep sutures lay above them and two below; each of these cords were tied in turn, with the full apposition of the cut surfaces one to the other. Such

being accomplished, five *superficial* sutures, silver wire, were inserted in the intermediate spaces, and so the entire wound, deep and superficial, was accurately adjusted, cut surface to cut surface. The waterproof cloth was now removed, and efficiently it protected the patient's flannel dress from the least wet. Large compresses of lint were laid at either side of the wound, and drawn towards each other by wide adhesive straps passed from one side of the abdomen to the other, affording very gentle equable pressure; a few folds of French wadding was placed over all, and retained in position by a many-tailed flannel bandage applied evenly from the pubis to the ensiform cartilage, and with an amount of support agreeable to the patient and protective of the parts within; the flannel drawers and jacket were next closed, and the patient, lifted horizontally from the table, was taken from the theatre to a small ward attached to it, and for the reception of patients after operation, and placed in bed upon a hair mattress. The bed had been previously thoroughly warmed with several jars containing boiling water, and the temperature of the room was moderate and refreshing; as she was placed upon her back the head was only slightly raised, and a pillow was placed beneath the hams, so as moderately to relax the thighs and legs; the hot jars, guarded with flannel bags, four on either side, were then placed close to the patient, and two to the feet, while a few coils of India rubber tubing, filled with hot water, were placed over the thorax, under the arms, around and upon the abdomen. During the greater portion of this time the patient lay under the influence of chloroform; and certainly nothing could be more satisfactory than its tranquilizing influence; from the beginning to the end she lay in a calm quiet sleep; on having recovered consciousness she had no remembrance of the operation, and did not suffer the least pain. Pulse being weak, ordered a table-spoonful of brandy and hot water every second hour, with five drops of Battley.

5 p.m.—Has rallied well since morning; skin perspiring freely; patient vomited several times, which I attributed to the chloroform. Stomach quieted by half a tumbler of hot brandy punch and twenty drops of Battley. Complained of pain also in the wound for two hours after the operation, but it gradually subsided, and at the hour visited was quite free from it. Vesical contents drawn off, full in quantity; ordered to be done every sixth hour. Jars to be renewed every five hours.

16th.—Slept well during the night; complained of heavy pain



below ensiform cartilage. Drew aside the bandage and applied a piece of lint damped with turpentine, which quickly removed it. Pulse good, 92; renal secretion abundant. Chicken broth, beef-tea, and two ounces of brandy, with water, to be given in small repeated quantities.

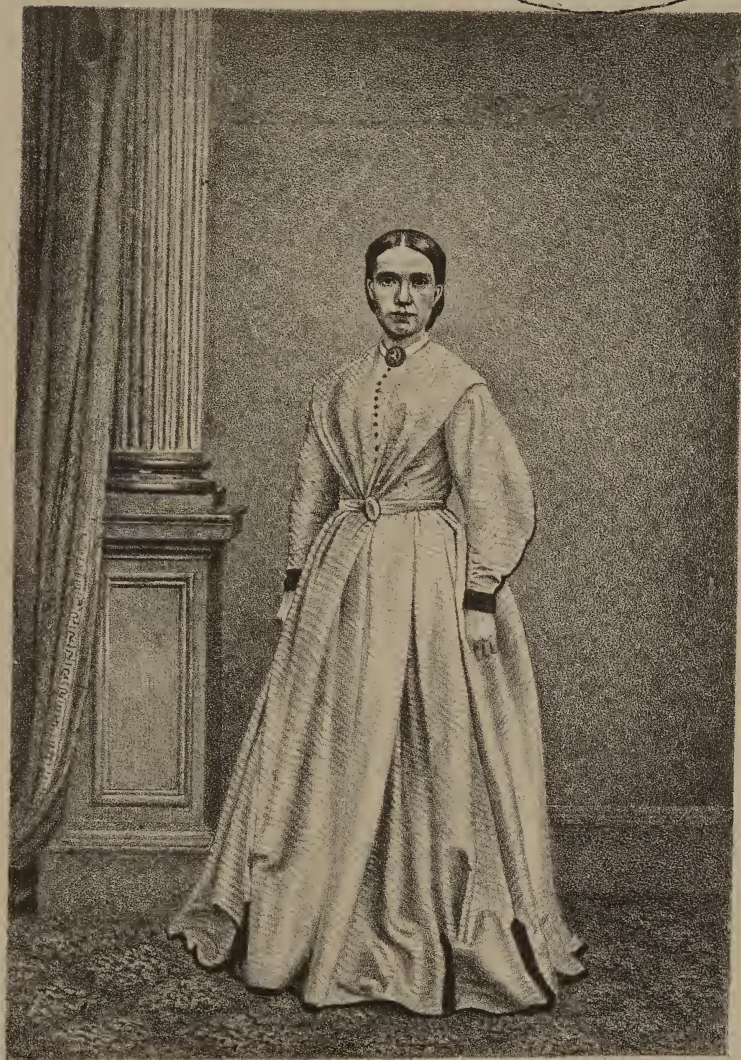
18th.—Slept well; no pain; pulse 90. To continue everything as on yesterday.

19th.—Had a quiet night; refreshing sleep; no pain; pulse 88. Looked to the wound; no redness about it—no angry discolouration—no pain on gentle pressure; edges united throughout, except the track through which the deep ligatures of the pedicle pass. Cut across the silken sutures, and removed each, beginning from above; the wound seemed as firm as ever after. Applied adhesive straps, compresses, French wadding, &c., as before, and again supported all with the many-tailed flannel bandage. Food; brandy to be continued. Administered a full turpentine enema, which acted most efficiently.

25th.—Went on most favourably up to this date, the symptoms and treatment being almost a repetition of what preceded; but on this day the patient complained of sense of oppression and sinking, deep pain across the stomach, and beneath the prominent ribs in the right and left hypochondriac region. She was blistered rapidly by the acetum lyttæ, and immediately after a warm linseed-meal poultice was applied, and full doses of chlorodyne given every third hour with entire relief. Wound discharging at the side of the pedicle ligature in small quantity, and matter healthy; no pain on pressure in its vicinity. Removed now the silver sutures; they effected their purposes in bringing about perfect adhesion, and seemed perfectly innocuous to the surfaces they gripped; being no longer required, taken away.

27th.—Sleeps well; no pain in abdomen or chest; pulse 86; soft compressible; skin natural; removed jars altogether. Nourishment—chicken broth; beef-tea—taken abundantly.

The patient rapidly gained strength, and proceeded most favourably; still confined to bed; and it was not until the 26th of May the ligatures of the pedicle came away, at 10 a.m., forty-one days after their application. Though for many days before, when I visited the patient, I twisted round and round, forcibly, the cords, and made gentle traction at the same time upon them, yet not until the above date, when practising this manipulation in them, did they show the slightest tendency to come away; no blood followed their removal,



Mr. Butcher on Ovariectomy and the after treatment of the patient.





and in four days after the track in which they passed was shut up, permanently obliterated, and the skin healed; still I confined the patient to bed for another fortnight, to make sure that no irritation should be got up, by too early exertion, in the newly united parts. During this repose, it was truly astonishing to observe the amendment in the girl's appearance. At this time, too, the catamenia appeared, showing that the remaining ovary was perfectly healthy, and affording indication of its functions. Soon after this the young woman was up and walking about, day after day gaining strength and flesh. No better instance could be adduced, I conceive, to illustrate the power of modern surgery to save, than that which I have but feebly described, yet with all truthfulness have endeavoured to map out. Who that had seen her, the poor, emaciated, sunken, dying creature, could have believed she was yet within the grasp of surgery, and to be rescued? Who that had seen her, unable to move at all, owing to oppression from the great morbid product, without a succession of faintings, would not wonder at the art that rescued and saved her, and restored her to her present condition, one of symmetry, activity, health, and usefulness.—(See Plate II., taken from a beautiful photograph, by Robinson, after the patient left the hospital.) I am happy to say that this young creature is now competent to follow her ordinary occupation, and has recently been enabled to accept the management of her special department in a large establishment.

CASE II.—*Enormous Unilocular Ovarian Tumour; Operation by Excision; Perfect Recovery.*—The following case is most interesting in all its details, and speaks loudly in favour of the operation. I was called upon by Dr. Churchill, August 18th, 1865, to see a lady, with him, labouring under ovarian disease. The patient was unmarried, aged thirty-eight, of dark complexion, and a sufferer for more than ten years. At first the tumour was gradual in its increase, then rather stationary for months, then increasing with great rapidity, while then for three years it made steady, slow progress, attended by some pain in the right iliac region, but not sufficient to prevent exercise, or excursions of pleasure into the country. During the last two years the rapidity of growth was most remarkable, and commenced with most acute pain towards the right of the umbilicus, and two inches lower when taking a long walk; from this time the growth rapidly increased up to the period of consultation. On seeing the patient



on the above date her condition was as follows:—She was a haggard, wretched, pallid, emaciated creature, oppressed to the severest degree in the respiratory and circulating systems; pulse labouring in its thready beat, and 120 in its minute number; catamenia stopped for several years. On examination of the abdomen an enormous globular swelling presented itself; the girth at the most prominent part at the umbilical level being four feet eight inches; from the ensiform cartilage to the umbilicus, fifteen inches and a half; from the umbilicus to symphysis, six inches and a half; from the right anterior superior spine of the ilium to the umbilicus, eleven inches and a half; and from the right anterior superior spine of the ilium to the umbilicus, eleven inches and a half; and from the same point on the left side to umbilicus, thirteen inches and a half. On percussion over the entire front and sides of the tumour a dull sound was given; and on palpation of the tumour a distinct undulating wave was transmitted, when struck either laterally or in the vertical axis; on percussion towards the posterior walls of the tumour a tympanitic sound was elicited on either side, diagnostic of the locality of the regional bowel; on examination, *per vaginam*, uterus free. On the manipulation of the tumour no evidence of adhesion could be discovered; and on the balancing, percussion and succussion of the tumour, no solid material could be discovered in its volume. In consultation with Drs. Beatty and Churchill, the case was considered suitable for operation, and on the morning of the 20th August, at 11 o'clock, I performed the operation, assisted by those gentlemen. Indeed there is but little deviation from the course which I pursued in the case of Goodwin. The patient was placed upon a table of convenient height, and supported as nearly as possible in the position most subversive of increased oppression or difficulty in respiration; to secure this the head, the shoulders, and the loins were gradually supported with pillows, and the legs permitted to hang over the end of the table and rest upon a chair. Dr. Gordon then administered chloroform, and brought the patient, without a struggle, calmly under its influence, and the waterproof sheet, with a hole cut in it, stuck upon the lateral walls of the abdomen, leaving all that was required revealed, and insuring the flannel dress in which the patient was clothed, as in the former case, from being soiled or wetted. Standing on the right side of the patient, I commenced the first incision one inch and a half below the umbilicus, in the centre of the linea alba, dipping the point of the instrument deep, so as to come down at once upon that structure; it was continued downwards for nearly three inches, the same

weight upon the knife, so as to perfect the section down to the lowest angle of the wound; it will at once be recognized that in this stroke of the knife the skin, the fat, and the superficial fascia, were cut through. In this instance it was most remarkable the abundant supply of fat on the abdominal walls, while the rest of the body and extremities were wasted to the lowest degree. The depth of the wound throughout, from angle to angle, was fully two inches and a half. The linea alba being exposed, a small opening was made in it, and a director introduced, first upwards, and the structure freely divided to the point corresponding to where the knife was first laid on the integument; the instrument was next turned downwards, and the division of the part accomplished to a like extent; the peritoneum next appeared, was taken up in a forceps, a small opening made into it, according to the rules I have before expressed, and freely divided to an extent perfectly equivalent with the first incision. I may mention here, on the division of the peritoneum, towards the upper angle of the wound, a considerable artery was divided; this was carefully, and at once, ligatured. The ovarian cyst next appeared, so different in its colour, in its structural arrangement, even at a glance, from that in the former case, that no dependence can be made, nor reliance on the supposition, and as it were, the stated aphorism "that once the ovarian cyst is seen it can never be mistaken." I must be permitted to dissent from this saying; in the first case detailed I have stated that the cyst was white, glistening, presenting apparently *considerable consistence*; in this instance, how different; the projecting cyst was a dusky colour, purplish, with congested venous trunks scattered here and there, while a kind of venous ramiform injection seemed to give colouration to the intervening spaces; and yet, with all, the cyst did not present the marked "consistence," as in the first instance recited; a good lesson may here be learned from the colouration of parts; the skilled anatomist and operator will fully estimate the natural structure of parts as he proceeds in his dissection, but he must be equally prepared to recognize the varying changes which morbid influences will exert upon living structures in aberration from health.<sup>a</sup> The sac being arrived at, the trocar used in the former

<sup>a</sup> In this case how serious would have been the consequences if the patient had been submitted to the ordinary operation of tapping; in all likelihood a fatal termination by hemorrhage. Experience and statistics go to prove that tapping an ovarian cyst is by no means free from peril, peritoneal inflammation sometimes rapidly setting in, and quickly destroying life; while death by hemorrhage, in the manner I have adverted to, must offer another objection to its being lightly undertaken.

case was plunged into the tumour, and quickly the fluid flowed through the India-rubber tubing into the bucket for its reception; as it moved away, gentle compression was exerted from above, and laterally, by a few turns of a flannel roller; and the sac was grasped with a vulsellum, and steadily, forcibly, drawn, as its contents diminished, and twice with very considerable force as adhesion restrained for a few moments its onward traject; yet by pressure from above, by gentle, steady, yet forcible traction on the tumour, these adhesions, though firm, yielded gradually, and without consequent hemorrhage; and the entire tumour was delivered, there being no solid matter in it, by this very small opening; so well did the fluid escape, not a portion of it, or blood either, got into the peritoneal cavity; and so this enormous pendulous sac hung supported without the abdominal cavity, its pedicle absolutely filling the small wound through which it was extracted. The tumour was held up, and the pedicle elevated and put upon the stretch in its transverse axis, and so its constituents were clearly revealed by the light; at least a transparent line marked its division into somewhat unequal portions, the right somewhat larger than the left; yet it afforded practical information as to where the part should be transfixed *innocuously*—a momentous point, *as it is known some patients have perished by internal bleeding, by a wound of some large nutrient vessel, by the sharp instrument conveying the cord*; again, I repeat, by the precaution adopted in both these cases of mine, as subjected to the light test, so unfortunate a consummation can be averted and avoided. A double silken ligature here was placed through the transparent part of the pedicle, its loop divided, and each portion tied with the greatest tightness, and doubly knotted; the double ends were left out of the wound until a saving ligature was thrown on the pedicle, which was extremely short, about three quarters of an inch away, and nearer to the tumour. I then severed the pedicle between those ligatures, as close as I could safely go to those which were to remain behind; not the slightest oozing occurred; by careful section the part was divided, and the tumour separated from the body; the ligatures of the pedicle were then closely clipped to the knots, and the part left at rest. The left ovary was lifted up, carefully examined, and pronounced healthy. The India-rubber covering being taken away, the wound was adjusted, as in the former case, by deep and superficial interrupted suture; the deep, of silk cord, four points in all, carried through the abdominal wall and peritoneum, and tied upon

the surface; the superficial, of silver wire, three points in number, twisted upon the surface, and so fastened; adhesive straps, pads, compresses, both of lint and cotton wool, arranged and secured by the many-tailed flannel bandage, just as in the case first detailed; and so likewise, the adjustment of the patient in bed, as to position, and the application of heat to the surface of the body, both by jars and tubing. The patient rapidly rallied from the influence of the chloroform, and was not conscious of having suffered pain. She took some wine and water, and then went off into a quiet sleep. At 3 p.m., gave a full opiate, as she became a little restless; at 9 p.m., reaction fully established; pulse 100; skin freely perspiring. To have chicken broth in small quantities through the night, and occasionally a tablespoonful of weak brandy and water. I am happy to say she had no vomiting, which so frequently sets in after the free exhibition of chloroform. Vesical contents drawn off every sixth hour, and full in quantity.

August 21st.—Had some sleep; yet restless, and talking violently; pulse 100; skin moist; the bottles being renewed regularly every four hours. Ordered draughts containing Battley and chlorodyne, in large doses, every third hour; the chicken broth and weak brandy and water to be continued as the force of the heart's action still depressed. 9 p.m.—Quieter; had some sleep; countenance not so anxious or distressed. To continue nutriment, and stimulants, and draughts through the night.

August 22nd, 9 a.m.—Was restless towards morning and wandering a good deal in her mind, attended with a good deal of excitement; surface of body moist; pulse, 96, a little hard; complains of pain in the wound on filling the chest by a deep inspiration; sickness also of stomach; wound, on being exposed, looks well, but tenderness on slightest pressure around it; ordered a grain of calomel and a grain of powdered opium every second hour; beef-tea in small quantities every third hour.

3 p.m.—Pulse, 94, somewhat softer than in the morning; renal secretion copious, and removed at the intervals already mentioned.

August 23rd.—Still lurking pain about the wound on deep inspiration; pulse yet hard; every third hour, beef-tea, &c.

August 24th.—Restless night; disturbed raving sleep; pulse, 86, yet hard; to continue the pills, one every third hour; beef-tea, arrow-root with port wine; brandy and water.

August 25th.—Slept well; pulse down to 84, soft; skin freely perspiring; *salivation* produced, and so constitutional symptoms



checked, and also the local pain, on deep inspiration, now gone; bowels opened three times freely; stopped the calomel, and ordered a grain of powdered opium every third hour, in pill; beef-tea, arrow-root, and port wine; and brandy and water as before (one glass of brandy in the twelve hours); exposed the wound; no pain about it even on gentle pressure; cut the deep silken sutures, and withdrew them; the wound nearly united throughout; suffered the silver wires to remain; re-applied adhesive straps—compresses as before.

August 26th.—Had a quiet night for a few hours; but at three in the morning called to wait upon her, owing to extreme torture and entire abdomen greatly distended with flatus; vomiting and great attendant depression and sinking; at once put a turpentine stupe on upper part of abdomen over epigastric region; and administered with long tube a full turpentine and oil enema, which brought away a considerable proportion of solid material, and gas in quantity; the tongue was dry, and pulse feeble; ordered brandy, six ounces, in small doses, every second hour; opium to be continued, beef-tea and ice occasionally to relieve the nausea and sickness.

August 27th.—Had some sleep; bowels freely opened again towards morning; pulse reduced in number, soft; skin freely perspiring; no pain, and tympanitis greatly reduced; at 2 p.m. better in every way; had quiet sleep; had partaken freely of hot milk and water, brandy and water; dressed the wound; some slight purulent discharge from lower angle; upper united for two-thirds down; removed the silver wire sutures; parts adherent all about them; some matter issues, upon pressure, through the holes where the deep sutures lay; 9 p.m., bowels moved freely, yet abdomen greatly distended; passed up the long elastic tube and permitted its end to rest in a basin of water; the large quantity of air that escaped was most surprising, reducing perceptibly the distended walls, and affording commensurate relief; to have four ounces of brandy, in water, at intervals, beef-tea in small cupfuls every third hour, and the opium as before, every third hour a grain.

August 28th.—Slept well; bowels well freed; tympanitis down; respiration freer; no pain on full inspiration; no pain over abdomen on pressure; pulse, steady, soft, 96; tongue clean and moist; wound looks well, some matter trickling up through suture wounds; dressed as before; to continue the opium, as the moment suspended, irritability sets in of a fearful character; to continue the stimulants, brandy and water, and the beef-tea, each every second hour.

August 29th.—Suddenly and alarmingly prostrated; pulse, feeble, small; tongue dry, brown; on examining abdomen pressed out a large quantity of the most fetid watery wheyish fluid through the lower ununited part of the wound, deep down in the cavity, and evidently the effused fluid rapidly thrown out on the supervention of inflammation of the peritoneum, but arrested further and then discharged through the wound—fetid, corrupt. The active mercurial and opium treatment arrested the continuance of its secretion, and the constitutional support and management of the patient rescued her from its deleterious consequences; and so, likewise, the local management determining its course and evacuation outwards. Dressing most carefully by pads and well-adjusted pressure, adhesive straps, and tailed flannel bandage; brandy and water freely; port wine and arrow-root; beef-tea and chicken jelly; one or other of these given every second hour, so great was the marked prostration; to repeat the turpentine enema and stupes, the abdomen being distended with flatus, and very painful.

August 30th.—Much the same as on yesterday; to continue everything, stimulants in greater proportion.

September 1st.—Better in every way; had a good night, quiet sleep; pulse, 84, soft, equable; tongue moist; skin freely perspiring; discharge from wound still copious and fetid; belly slack; to have arrow-root and port wine, chicken jelly, brandy and water every second hour; omit now the opium, their being no pain in the vicinity of the wound on pressure or otherwise.

September 3rd.—A very large secretion of most offensive thin purulent matter issued from the wound on pressure and this state was ushered in by restlessness, flushed face, yet all the disorganized secretion readily was evacuated through the lower open angle of the wound; stimulants increased and food as before—as much as could be partaken of.

September 3rd.—Fetid matter diminished and condition of patient improved; to continue stimulants and nutriment.

September 10th.—Had sleep, but flushed (painted cheek); dreamy night; pulse a little accelerated; tongue dry; I suspected something astray near wound, and found towards its upper angle a hard round space about half again as large as a crown-piece, and over it the skin thin and giving evidence, by its condition, of the mischief beneath; I applied over the entire abdomen a large linseed-meal poultice, and with great comfort; this covered with layers of wadding and supported in position by the tailed flannel bandage;

stimulants and nutriment as before—one or other every second hour; thus the drooping powers of life were sustained. By this mode of treatment, local and constitutional, the dangers have been passed over; now, the 14th September, she sleeps well, and followed by refreshment; her pulse is only 74, soft, calm, equable; all flushing heat, and perspiration gone; tongue clean, moist, and of natural size; digestive and urinary organs acting healthily and naturally; the abdomen looks well; discharges small healthy pus, from two points at the upper angle of the wound, rather free still; and at the lower angle thinner in its character, not so satisfactory, and clearly afforded from a source different (probably the peritoneal cavity) from that in the superior region of the wound; a linseed-meal poultice laid over the abdomen night and morning after the matter is gently pressed out; all distress from countenance gone, getting up flesh, and now constantly demanding food.

16th September.—Greatly improved in every respect; tenderness over the abscess gone; all discharges, though very free for the last few days, now greatly diminished, and of the healthiest character; pulse, full, steady, and 74; patient able to turn on either side without the least pain; takes stimulants and food abundantly, and with appetite; wound still dressed with the linseed-meal poultices, and flannel tailed bandage support.

21st.—Has rapidly and steadily improved in general health; pulse 72—soft, natural; appetite excellent; the discharge from the abscess now almost gone, not more than a few drops of serum passed out; the constricting ligature threads have never appeared in the discharge, though cautiously watched for; all constitutional disturbance and local tenderness gone; it is very remarkable with what rapidity the fat is being deposited, and the rotundity of the body restored; but not so surprising as the altered haggard countenance so quickly replaced by one of cheerfulness, exhilaration, and expression. At this moment the patient is most anxious to get up; and if her general appearance and ability to do so was alone considered I am quite certain she is competent to the task; but it should never be forgotten, in those cases, cases of recovery and cure, that extensive adhesions have been set up, created, as being essential in guarding their recovery; now these adhesions, so recently formed, so imperfectly, at this stage, vitalized, may by any rudeness be rapidly disintegrated, broken up, and so giving rise to a train of constitutional symptoms intolerant of restraint, and so becoming fatal; for these reasons, these alone, the patient is

still restricted to bed; however, she sits up in bed for hours during the day, propped and supported by pillows.

On September 30th, the patient was up and walking about, perfectly free from inconvenience. As in the former case, so in this, the rapidity with which power and tone have been restored to the system; the whole countenance and caste of the patient has undergone a marvellous change; the pallid sunken face has resumed its former fulness, cheerfulness, and happy expression; and she who only a few days since was numbered with those on the brink of the grave, is now restored to her family and friends with as good a prospect of lengthened years as any one of them; and so—conservative surgery ever stretches forth its arm to save.

The history of these successful cases, this triumph over disease, otherwise progressive unto death, will, I trust, give more confidence to the timid in the adoption of this saving operation. To the vacillating practitioner I would speak, and remind him that the united testimony of the ablest physicians go to prove that all medicines prove abortive here; and the sooner the operation is performed, when the functions essential to life are impeded and interfered with, the better; I have no hesitation myself in recommending early operation when once the case is truly diagnosed; the earlier, the less chance of adhesions; the earlier, before the reparative powers of life are taxed, the more certainly may be looked for adhesive inflammation; yet, if the tumour be slow in its progress, or as sometimes will be the case, arrested for a time in its development, the surgeon should pause—not meddle or hazard life; again, I think the operation should be deferred during the incipient stage of the disease, as the peritoneum will be intolerant of it; I have no doubt whatever that after the affection subsides for some time, great changes are wrought in the peritoneum, as to its resisting powers of inflammation; it is not nearly so great when the abnormal growth has impressed the influence of its presence upon it for some time; it seems as it were to be gradually dulled in sensibility and active vascular action; in no other way can the violence inflicted by some operators on the membrane, almost with impunity, be accounted for; the tearing through of surface adhesions, the crushing by the *écraseur* of others more developed either in surface or length; the ligaturing of others, and the clipping away by scissors of some, all point to the same fact that the membrane at this time is spoiled in its sensitive inflammatory idiosyncrasy, so characteristically inherent in all serous membranes.



There are a few points relative to this operation that I would briefly insist upon.

The case being judiciously selected, I am persuaded it is a great error to operate in an over-heated room; the fashion being to exalt the temperature of the apartment far above the ordinary standard, or that adopted for comfort. The first case which I operated upon I brought into the theatre of the hospital, which was just of a comfortable temperature, not being varied a single degree more on this occasion than on other days for ordinary operations; numbers of practitioners and pupils were present, and none complained of the spacious room being hot; I think it is a most important matter affecting the result of the operation that the room should be cool, and hold a large quantity of pure air in it; with full proportions of its constituents for healthily oxygenating the blood; it should never be forgotten that the patient subjected to this operation is to be rendered insensible by chloroform, and probably maintained so for a long time; that when brought into that room she is living on a very imperfect aëration of her blood from mechanical impediments;—the cold body surface, the feeble pulse, the livid lips, all bespeak this condition; is it then reasonable to suppose, under such conditions, that the patient can, with impunity, be refused a due supply of well-oxygenated air; is it to be supposed that the shock of operation, the revival from the congestive effects of the chloroform, will not be grievously, nay, dangerously prolonged by an absence of it.

Next comes the placing and arrangement of the patient on the operating table. The patient should be drawn down towards the end of the table, with her legs hanging over it at right angles with the thighs, and the feet suspended on a shelf drawn out from between the legs of the table, or resting upon a chair cushioned to a suitable elevation; leniency then must be ceded to the patient as to the elevation of her shoulders, neck and head, at the same time due support must be placed beneath the loins; now why do I rest upon these minute particulars? Because, it must be remembered, the patient is struggling with embarrassed respiration, and particularly so when in the recumbent position; she must be allowed to seek the position that will facilitate her obtaining as full a quantity of air as possible at the same time compatible with the surgeon's task; and the necessity for doing so is further enforced, because chloroform has yet to be administered, and its influence impressed in a complicated operation, probably for a long time; from

such circumstances I deprecate the practice of administering the chloroform in an adjacent room to that in which the patient is to be operated on; and then, when insensible, placing her upon the operating table, without knowing how far constrained position is compatible with her safety.

Now as to the steps of the operation. As illustrated by the foregoing cases; an enormous tumour may be removed through a wound of not more than two and a-half or three inches in extent; so the incision should not at first exceed this measurement, commencing at the point referred to below the umbilicus, in the centre of the linea alba, and carried downwards to the prescribed distance; the knife should sink to the same depth from where first laid on to the termination of the incision, and throughout its entire track it should cut through the skin, fat, superficial fascia, down to the linea alba; this structure being brought into view, its most projecting prominent part—and that will be found about the juncture of the lower with the two upper thirds of the line exposed; the fibrous bands should be lifted up with forceps, and a small opening made, the blade of the knife, horizontal to the surface, and division made sufficient to admit a director, which should be slightly curved previous to its introduction; this being well held up to the under surface of the linea alba, the knife pressed upwards along the groove of the director, effectually severs it, so then the director is turned downwards, and the part slit to the lower angle of the wound; the peritoneum next presents itself; this should be cautiously lifted up by a forceps, and the knife horizontally held to the surface, made to cut a small aperture in it, after the manner adopted in opening a hernial sac, as the linea alba was slit from one end to the superficial wound to the other, so likewise should the peritoneal membrane be cut; caution must be adopted in recognizing the peritoneum from the ovarian cyst; for instances have been afforded where the practitioner had presumed he was down upon the cyst when in reality he was outside the peritoneum, and vigorously separating it from the abdominal walls, when he was exulting in the satisfactory way in which he broke the tender adhesions binding the tumour to surrounding structures and neighbouring organs.

From the foregoing cases it will be seen that the colour of the cyst cannot be depended on as diagnostic of its presence; in the first case which I operated on it was of a peculiarly white colour; in the second, turgid, with blood-vessels, venous in their character, and permeating it everywhere; yet it may be relied upon that the

colour of the ovarian cyst, in the large majority of cases, bears a close resemblance and analogy to the colouration first insisted on. In the superficial wound bleeding is not likely to occur; in the division of the peritoneum it is not unlikely that a branch of the epigastric may be cut, as occurred in my second operation; and if so, in either case, the vessel should be ligatured at once. The sac being fairly brought into view throughout the entire extent of the wound in the abdominal wall, the trocar, projecting from its sheath, should be thrust deep in, with decision, up to the hilt, when, the pressure from the trocar being taken off, it recedes, the fluid following it to the opening in the side of the canula, and then, through the India rubber tube there attached, into the bucket for its reception, and placed there previous to the operation being commenced. As the fluid flows away so the sac shows puckering and signs of collapse; then it should be seized with a vulsellum and drawn steadily through the wound, when a small portion of it escapes; pressure, by an assistant, on the abdomen above, by a constricting belt of flannel, very gently, governs its recession in the direction of the wound; and these combined forces, gradually, yet determinedly enforced, will overcome many, and even extensive, surface adhesions; the giving way, the tearing up of the connecting medium will be appreciated and readily recognized by the hand and touch of the surgeon as the sac is being liberated, and the exposed sac delivered will give ocular evidence by being extensively ruffled on the surface; these two points were clearly represented in each of my two cases; therefore I would lay it down, as a rule, *that neither the hand, director, or any other foreign body, should be introduced into the wound to seek for adhesions until the attempt at its delivery be made after the manner I have just described.* I am induced to lay stress upon this point, because, in the second case which I operated upon, very firm adhesions restrained the growth; yet, by determined forcible traction, they gave way in the most satisfactory manner; I am certain that this measure, if carefully put to trial, will be found effective in three-fourths of the cases that may be considered suitable for operation; if not, why no harm has been done; and then the wound can be enlarged, the adhesions sought for and liberated, either by rupture of them by gentle tearing, division of them by the *écraseur*, ligaturing of them by silver wire, and then cutting external to the pedicle secured; if hemorrhage occurs after either of those measures, a touch of a heated flat cautery will restrain it; or a pledget of lint, soaked in a

saturated solution of perchloride of iron, held for a few seconds to the weeping surface will be effectual in restraining it; if a large vessel should yield blood it should be tied with a thin silver wire thread; if the sac in any one part should be found, as it were, fused into the surface of a vital organ, it would be well not to use any rash violence, but, when the sac was entirely emptied of its contents, leave the adherent portion behind, and clip away the rest; by this method no risk or serious mischief would be inflicted. The tumour being delivered, and sustained in the hands of an assistant, the important question comes—that of dealing with the pedicle; great variety of opinion prevails on this point; and I do not think it is to be wondered at, as the recorded experience and testimony of many go to prove that the constituents of the pedicle are subject to much variety in thickness, massiveness, and vascularity, as, likewise, in its length—in some being short and truncated, in others attaining to four or five inches; in the first case which I operated upon the pedicle was somewhat more than two inches and a-half in length, and very thick and massive; as a temporary measure I applied a ligature close to the root of the pedicle, and another far away, and then, with a wedge-shaped cautery heated to the proper temperature, cut steadily through its component parts; yet, though the pedicle was tied pretty tightly, blood rapidly flowed—pure, bright, arterial blood; then the pedicle was held up to the light, and a large artery dissected away and ligatured—that which the cautery could not control; the remaining portion of the pedicle, being held up to the light, was sufficiently transparent in some places as to exhibit where the sharp instrument might be introduced, conducting the double ligature, without the least risk of transfixion or division of a vessel; thus the pedicle, being divided into portions, permitted the cord to effectually strangle each included part, and so secure its certain death; for, it should be remembered, if the part included in the ligature is too thick, the counteracting force will not be efficient to secure its death, and after a few hours the ligature becomes lax, and hemorrhage may occur from some vessel not occluded in the centre of the part embraced; the ligatures securing the split pedicle, as well as that upon the large artery, were brought out through the wound, perfectly lax, no dragging on the pedicle, and were not cast loose for many days; yet very little discharge followed in their track.

In the second case which I have detailed the pedicle was very short; and, though thick when taken up, yet could be spread out,



held up to the light, and made transparent in some places—and, further, even divulging the passage of the blood-vessels, large and numerous; here the pedicle was transfixed, in a suitable position, with a sharp-pointed eyed needle, in a handle, conveying a strong double silk ligature; each half of the pedicle was fairly and tightly strangled with great force; then a ligature was placed round the pedicle, nearer to the tumour, and section made close to the double ligature; and so the abnormal growth was severed from the body; not a drop of blood oozed from the constricted pedicle; in this instance both ends of each ligature were clipped close to the knots, and the constricted parts suffered to retract and be at rest. Though abscess formed in the vicinity of the pedicle, with hard firm cyst, and liberating its contents through the middle of the wound, yet these ligatures never appeared, though carefully watched for in the discharge, and all healed up within a month.

From the first case it would follow, when the vascular supply is through large channels, the cautery cannot be depended on, and the ligature, to be effectual, must strangle the pedicle, if thick, in portions; it might be argued, too, that the track of the ligatures externally directed secretions with safety from within. In the second case, where the pedicle was strangled in portions, and the ligatures clipped close to the knots, though a small abscess formed, yet it was obliterated, and the wound healed as rapidly as in the first case. I am convinced the surgeon who rests on one plan will not be as successful in his practice as the man who adapts his measures to the peculiarities of the case as revealed in his procedure. No doubt, if a number of small vessels pass through the pedicle for supply and distribution to the tumour, I admit the hot iron may be competent to sear up and close their divided mouths; but I deny its power to close with security larger vessels; it was incompetent to do so, though applied at the proper heat, slowly and cautiously, in the first of my cases; and, on examination of the preparation lodged in the Museum of the Royal College of Surgeons, a solution of the difficulty in its doing so is at once revealed. By the ability of Dr. Barker, curator of the museum, this specimen has been prepared; it is most beautifully injected, and distended to its full proportions; the vessels are as large nearly as the little finger; then, would it not be futile to depend upon cauterization here? Now, in my second case the pedicle was very short, and seemed a congeries of large vessels; and it was with difficulty, when held up to the light, to secure a point for the sharp ligature-conducting

needle to transfix with immunity; neither in this instance do I think the cautery would have been effectually preservative; and the vascularity of the sac—dark, turgid, and unnatural to its kind—is an evidence of the multitudinous vessels which sprung from the pedicle supply; this beautiful preparation is also lodged in the college. I do not think the cautery should be depended on as a sufficient preservative in these cases; it may have succeeded in instances where the vascular supply was small, and given grounds for confidence; but I must enter my protest against the practice when the vessels are large. Now, I do not wish to disparage the power of the cautery; I use it extensively every week; and in all those cases where the pedicle is ligatured piecemeal I would recommend the appliance of a quick brush of the cautery over the cut surface; I have found its application in this way most serviceable after excision of the upper jaw, and other severe measures, in arresting unhealthy forms of inflammation, diffuse or erysepelatos, and arousing at once a reparative inflammation; and it is my opinion its judicious application to the cut surface of the pedicle would be followed by a like success. I would reiterate here, in proportion to the bulk of the pedicle it must be ligatured in two, three, or even four portions, so that each constricted part shall surely die; the little noose or knot of silk will find its own escape. Strong silver wire may be substituted as innocuous to surrounding parts. Now, as to the practice of bringing the pedicle out through the wound, and retaining it there by ligature or by clamp, no matter how well devised, it is needless to dwell upon. Any traction or straining upon the strangled part must be productive of mischief. I admit the inconveniences set up by such practices may sometimes, by prompt, judicious treatment, be stifled and subdued, and then a false impression is arrived at as to the salutary influence of the measure. If the operation is to be fairly carried out according to my views, the pedicle, when once protected from hemorrhage, by either cautery or ligature, must be unrestrained, at rest, relaxed. As to the dressing of the wound, the mode I adopted in both the foregoing cases was the same—interrupted suture, deep and superficial, the deep stitches including the peritoneum; the deep traversed by silken cord, the superficial by silver wire; each acted admirably in sustaining together cut surface to cut surface, even almost to the perfect union of the entire extent of the wound; yet I was not satisfied with the reception of the silken cords in their long transit; small abscesses formed in the track of each, lymph boundaries

fortunately limiting them; yet, though safely walled in, creating distress and a remarkable amount of discharge; in the course of the silver wires there was no hardness, no lymph walls, no discharge. I shall in all other cases treat the wound on the same principles which I have advised for that in the operation of hare-lip; the needles, of course, must be made considerably longer, firmer, and thicker; but they should be constructed in the same relative proportions, their points triangularly shaped, and the opposite extremity blunted; the same rules, too, should regulate their introduction and transfixion of the abdominal wall; in proportion to the thickness so likewise farther away from the cut surface should the needle be entered, and then made to travel, from left to right, down to the peritoneum, and then its point appearing should be made to strike the right or opposite side, at a corresponding point as to arrangement of tissues, and, being pushed onwards, it appears through the skin at a distance from the wound equal to the point of entrance; the number must be according to the length of the wound; the distance between each should not exceed three-quarters of an inch; the walls of the abdomen being so relaxed, the edges of the wound can be brought, with facility, easily in contact by gentle pressure; and then the silken cord being thrown round the needle, in the figure of eight form, will prevent the slightest retraction; each needle must have its silken cord, irrespective of the one above or below, and so all puckering of the wound and undue tension on the needles be guarded against. I am convinced this is the most rational way of dressing the wound; it secures the surfaces (even in their deepest track) throughout in contact, and it excludes from injury by puncture the peritoneum. As to the superficial dressings—adhesive straps, tailed flannel bandages, &c., their arrangement has been already laid down.

Now as to the after-management of the patient. The sickness and vomiting which so frequently sets in after chloroform, though preceded by no food for hours before its administration, yet is sometimes very distressing; it will be best subdued by a warm stimulant—nothing better than half a tumbler of hot brandy punch—after which hydrocyanic acid, in three-drop doses, will act as a specific. In these cases I think it is a great error to keep the patient low; moderate stimulants and supporting nutriment—beef-tea should be given from the first in small repeated doses—cold brandy and water; ice to suck constantly to moisten the dry mouth, so intolerable after the chloroform, and persistent from the deranged sunken

condition of the patient before the operation. In addition, the temperature of the patient must be watched; the hot jars constantly renewed, for as yet the shocked system and feeble low respiration have no power to generate heat; thus the urgency of its artificial supply; likewise, the full supply of pure oxygenated air to revivify the blood, and so impart tone to the nervous centres, is absolutely demanded. If the surgeon's work throughout the early steps of the operation be carefully and efficiently performed he has no dangers to apprehend as consequent upon them—no likelihood of hemorrhage. The great danger which looms in the distance, even from the moment when the knife was first laid on, is inflammation—inflammation of the skin of an erysipelatous character, as I have seen from other wounds of the abdominal parietes—inflammation of the deeper tissues, of a diffuse character, as I have often witnessed—inflammation of the peritoneal membrane, as admitted and recorded to be so frequently the cause of death after this special operation. Peritonitis should be carefully watched, for here it is very insidious, owing to the low condition of the vital powers of the patient; pressure over the abdomen will not afford the earliest evidence of its presence; neither will the pulse tell; the respiration of the patient can be made by interrogations, the best exponent; if, on being asked to fill the chest, the patient complains of uneasiness, of pain in the wound or its vicinity, the practitioner may take the alarm; then, if pressure be made deep around the wound, soreness will be complained of; there may be no sickness of stomach to indicate this first, this early mischief; and the pulse is no safe guide; the pulse is quite deceptive; and, if depended on, will lull the practitioner into false security; once awakened the symptoms soon unmask themselves, and then the life of the patient is really in jeopardy. The question that suggests itself is of vital importance now; upon the judgment and decision of the surgeon the only hope of the patient's salvation rests; is the patient to be bled? some laud it beyond measure; I say it increases the chances of death; some may endeavour to make a division of the type of inflammation here, and call that which early occurs acute, and that at a farther off period typhoid; but this is an erroneous separation of the results of diseased action; the early peritoneal inflammation is asthenic; the late form is asthenic too, and but the evidence of complication of the worst kind of blood-poisoning; in either the taking away of blood is a deprivation of a part of the life that remains. The whole reliance of the practitioner here must rest upon the power of calomel and opium—



calomel in grain doses, with a grain of powdered opium, every second hour, with stimulants and nutriment; brandy and cold water every second hour to wash down the pills, and beef-tea every third hour in small cupfuls. The whole inflammatory action here is low, rapid, and destructive; therefore the imperative demand for stimulants to support the flagging powers of life until specifics have time to determine their influence.

The second case which I have described affords a good lesson of what should be done when life is urgently threatened by this peritoneal affection; here the disease seized on the part, prostrated its victim, as if stricken with the lowest typhus; by rapid exhibition of calomel and opium, by the abundant exhibition of stimulants, the disease, after a little time, was checked, and finally arrested, not however until the second act of inflammatory process was accomplished, but it was arrested even now by the persistent exhibition of the calomel and opium; the exhibition of the opium was long persevered in, even in large doses, when the calomel was discontinued after its specific action had been gently established. The inflated and painful condition of the abdomen was relieved by large turpentine enemata delivered through the long tube, turpentine applied on lint over the epigastric region, and warm linseed-meal poultices over the wound and iliac regions; shortly the adhesive lymph binding the lower angle of the wound gave way and out gushed a large quantity of pent up horridly offensive fluid, the result of the inflammatory mischief that threatened life, from day to day the discharge diminished and lastly purulent matter alone came away, of a healthy character, this, too, gradually ceased, and the wound healed. I trust, in the history of these cases, I have grouped the facts so as to present them in the most attractive form; I know I have omitted nothing that could be looked on as clinical in their instruction and teaching, and I trust, as I foreshadowed in the beginning, that the glorious result obtained in both instances—the saving of life, the restoration to health—will give confidence to the vacillating and to others in the prosecution of this noble operation.

ART. XI.—*Observations on the Epidemic Fever prevailing in Cork.*

Read before the County and City of Cork Medical and Surgical Association, by DENIS CHAS. O'CONNOR, A.B., M.D., Professor of Practice of Medicine, Queen's College, Cork.

THOUGH it cannot be said that fever has prevailed more in Cork than in other localities, it has never existed as an epidemic in Ireland without making its virulence felt in this city. An admirable description of the disease, as it appeared in this city in the middle of the last century, is to be found in the writings of two eminent Cork physicians—Drs. Rogers and O'Connell. Since the beginning of this century we had a severe visitation in 1802, when the Fever Hospital was founded; afterwards in 1817-18; again in 1826-27. In 1832, Asiatic cholera seemed for a while to have eradicated every other disease, and we had an exemption from fever till 1836-37, when it broke out in a mild form. It re-appeared in 1846, and became intensified in 1847, in consequence of the famine. It was, however, an error to call the fever of that year "famine fever," as the peculiarities attributed to the famine existed the previous year. The famine did not produce the fever; it only rendered a greater number susceptible of its influence. After the subsidence of the epidemic, intermittent fever made its appearance, and as long as it lasted scarcely a case of continued fever was seen. As soon as the last cases of intermittent disappeared, the present epidemic broke out, and still rages with much severity. This alternation of continued and intermittent fever is remarkable. Indeed it might have been observed that the fever of 1847 passed first into a remittent form, and gradually into the intermittent, which prevailed more or less for ten years subsequently.

From this short sketch it is manifest that although fever is more or less indigenous in this country it still appears in an epidemic form only after long intervals. It might be said these outbreaks of fever originated in the chance circumstances of prevailing distress, as in the years 1748, 1802, 1817, 1847. But the other epidemics referred to could not be traced to such a cause, and the present one occurs at a time when the food of the people is most abundant, and of the best quality. It is also said to be the direct emanation from cesspools, or other malarious influence; but in contradiction to this it may be observed that while this cause is constant, the effect attributed to it is most variable; and, besides, the particular form of

fever said to be specially dependent on this cause—typhoid—has scarcely appeared in this locality, which cannot boast of the excellence of its sewage. We must, therefore, seek for some cause. atmospheric or telluric, different from those which come under the cognizance of our senses, or which are discoverable by chemical analysis, to account for the outbreak of fever at particular times, Sydenham used the term, “*constitutio æris*,” to express this unknown property in the atmosphere. But fever spreads more slowly, and is more capricious as to the localities it invades than could be expected if the poison were diffused freely in the atmosphere. For instance, we find one town assailed, and a neighbouring city remaining exempt for a long time after; and even when a town or city becomes the seat of the disease, all parts are not assailed together, but those parts first attacked become first freed from the poison. Many striking examples of this fact presented themselves in 1847. Amongst others, the Cork Workhouse—where every third person was a victim of the disease, in the Winter and Spring—became entirely exempt from it in the Summer, the time at which other parts of the city were virulently assailed. In attacks of influenza, on the other hand, we have an example of how rapidly diseases purely atmospheric spread; as this assails not only all parts of a town simultaneously, but is diffused at the same time throughout the whole country. From all this we might infer that although the atmosphere, or some emanation from the soil, is capable of producing a tendency to fever at times, there must be some immediate cause capable of bringing it into activity in each locality. We might compare it to a house filled with combustible materials which, nevertheless, remain innocuous till something is brought into contact with them capable of producing ignition. The primary and secondary cause in this development of the disease are as much beyond our comprehension as the essential nature of the poison itself. The contagious character of this epidemic has been exhibited in the death of two of the medical attendants at the Fever Hospital, and of two dispensary physicians. Still equally strong proofs have been afforded that the poison emanating from the bodies of the infected is rendered innocuous by the slightest dilution with pure atmospheric air. In the large number of cases which I have attended I never found the disease spread from one member of a family to another, no matter how close affection or duty brought them into contact; provided the house was managed with the least attention to proper ventilation and cleanliness.

I shall now endeavour to describe the leading symptoms of this disease—no easy task, seeing how much they vary in different cases. The disease is generally ushered in by rigors, though in many cases so obscure are the early symptoms that they escape the observation of the patient, and mislead the most vigilant physician. The pulse, during this time, is often little above the ordinary standard, and the heat of the skin not perceptibly raised above that of health—sometimes is below it. The tongue also misleads, as it is seldom much furred at the commencement, and sometimes not much so through the entire disease. Sometimes, at this stage, the disease has been masked under the form of bronchitis, diarrhea, severe muscular pains, resembling rheumatism; and generally the causes which produced the fever were such as would have originated these diseases if fever did not prevail. So singular and unexpected were the transitions into fever, at times, that I was often led to think that it was a true change of disease, such as we often witnessed in cases of Asiatic cholera.

About the sixth or seventh day the disease passes into unmistakable fever, sometimes imperceptibly, but often with a sudden and alarming change, attended with vomitings, violent headache, slight delirium, and a sense of alarm to the patient. About this time the pulse ranges from 100 to 120, fever heat is established, the tongue is furred, and a whitish crupous-like exudation is often seen on the arch of the palate, at one or both sides, and sometimes on the side of the tongue, which I believe to be true typhoid deposit, as it appears and disappears with the cutaneous rash. It might be easily brushed away, but will as quickly reappear. Now and again the whole tongue is covered with this matter, resembling the thrush of childhood. During this period of increased febrile excitement the rash makes its appearance, and, as in the exanthemata, is often followed by a mitigation of the symptoms. In the character and appearance of this eruption there is a great variety. Sometimes as early as the third or fourth day there is a dark-coloured mottling, resembling the figures of *rubeola nigra*, appearing and disappearing several times, and about the eighth or tenth day giving place to some more defined rash. This eruption was never seen here in the fever of 1847. In other cases we find the body covered with fiery red spots, resting on a dark-red, ill-defined base; the latter extending and becoming darker as the disease advances. Sometimes morone-colour patches, slightly elevated, are diffused over the body. In all these the rash, like that of typhoid fever, is effaceable



on pressure, except in some cases towards the end, and begins to fade about the eleventh or twelfth day, even though the symptoms of fever have not yet left. The true typhoid rash has been seen but seldom, and the petechiæ of genuine typhus, so frequent in former epidemics, has been equally rare. The latter fact I attribute to the improved condition of our poor in good clothing and the ventilation of their dwellings. The absence of typhoid is not so easily explained, particularly if we are to adopt the theory that it is the direct result of the emanations from defective sewage, as Cork cannot as yet boast of much superiority over other towns in this respect. The general practice is to give the name of typhus to all fevers attended with a rash which is not of a typhoid character. Is this division useful in practice? The name typhus carries with it the idea of great prostration of the functions of the mind and body, nervous debility, and such alteration in the capillary circulation as leads to stagnation of the blood and all its consequences. In these respects the great majority of our cases more resembled in character the symptoms attributed to typhoid fever; exhibiting till the very last stage a degree of bodily and mental energy inconsistent with the blood-poisoning of the former disease. Names would be of little consequence if they did not carry with them erroneous ideas of practice both with the profession and the public, and in this case may lead to the too early and indiscriminate use of stimulants. In the subsequent part of this disease it sometimes, but rarely, assumed the character of typhus attended with great prostration, muttering delirium, and the gradual extinction of all intelligence. In the majority, however, the disease progresses without the intellect becoming much disturbed; or, if disturbed, the delirium indicating more an excited than an oppressed brain. In unfavourable cases the pulse and breathing become extremely quick, and the former very weak. This condition of the circulation precedes, and appears to be the cause of passive congestion in the brain, and œdema of the lungs, which bring about the fatal termination when this occurs. In comparing this disease with that of 1847 we note many points of difference. In the present we have no instances of relapsing fever, which was the prevailing type in the former period, especially among the poor. The present disease terminates generally by a single crisis, about the fourteenth day. Some few cases of a chronic character came under my notice, extending in two instances, to eight or ten weeks; but such cases were very rare. In the former period, diarrhea, with ochre-colour or pea-soup-

like discharges, occurred in nearly every case. In this there are few in which the bowels are not more or less confined. The biliary organs were more effected in the former, giving the skin an appearance of a yellow fever; in this the lungs and sympathetic nerve appear to be more influenced by the disease. In 1847 there was no eruption on the skin in many cases; whereas its absence in this epidemic is very rare. In the former the sequelæ were, chronic diarrhœa or dysentery, cold abscesses, œdematous legs; in this we often find great nervous debility, leading to a semi-paralysed state of the limbs, congestion, solidification, and sometimes gangrene, or suppuration of the lungs.

I would have some delicacy in expressing my own views of treatment in this complex disease; but as I have had the advantage of consultation with nearly every physician in Cork, I can state what has been the general practice in this city. When it is apprehended that a fever is about to set in, the usual diaphoretic medicines are administered, with mild aperients. Even at the earliest stage we are unwilling to give drastic purgatives, although patients frequently crave for active purgatives, to get rid of some very uneasy sensation which they complain of in the abdomen. This period is also availed of to administer some mild mercurial as an alterative. Leeching the temples, so common in former years, is seldom resorted to, and I am sure, in many cases, the omission is injurious to the patient. The great relief I have seen to arise in some cases from epistaxis induced me to apply leeches in others, who had similar symptoms, and I feel bound to say, with the happiest effect. In the whole matter of depletion we are suffering for the sins of our ancestors who carried this remedy too far. But I believe we are ourselves yielding to a form of public opinion created partly by a few scientific physicians, but more by homeopathic and hydropathic quacks and their admirers. In my earlier days we dare not treat a case of fever without some depletion. The same treatment if adopted now would get the credit of every bad symptom which may afterwards arise in the case. In this matter I am not "*laudator temporis acti*," neither do I sympathise with the "*avidî rerum novarum*," too numerous a class in the profession at the present day. About the sixth or seventh day there is frequently a struggle of nature for the development of the rash, as in the exanthemata. Stupes to the feet, and diaphoretics more or less stimulating, according to the particular case, favour that effort; after which the sensations of the patient become less

uneasy, either from an actual improvement, or from an increasing torpor of the nervous system. From this time forward, if not sooner, it becomes necessary to determine if stimulants, and of what kind and quantity, are to be used, also the amount and kind of nutriment the patient is to receive. In this epidemic there are few cases in which wine is not administered in greater or less quantity, from a tablespoonful every three or four hours, to the same quantity every hour. The object of giving wine is twofold—to act as a stimulant in sustaining the heart's action, and also by combining it with other articles of diet, such as arrow-root, that it might act as nutriment. With this latter object milk is urged on the patient in every way he can be got to take it—in the beginning in the form of whey, and in the latter stages mixed with water. Giving chicken broth in fever, is in this locality a great innovation. It was very seldom given in any former epidemic in this city till convalescence had commenced. Has the practice of the profession changed so materially, or is it the disease not the practice that has changed. I believe the latter. In most of the cases which we met in former epidemics the tongue was dry and thickly coated with fur. In the present it is moist and sloppy, showing that the stomach does not now sympathise in the disease as much as it did then. The best proof of the propriety of administering this nutriment is that it is relished and approved of by the patient. The case which is least fitted for chicken broth, that in which the patient cannot digest it, is that in which wine is most necessary as a stimulant, and *vice versa*. It is where the powers of the digestive organs are so impaired as not to be able to supply new blood to nourish the wasting tissues, and where actual blood-poisoning is weakening the heart's action, that the full action of wine is required as a stimulant. At the end of a fever thus fed we find the patient little fallen away—in fact a proof that his food had been well digested; while in a case of typhus with dry parched tongue, no matter how much wine might have been used, the patient was emaciated at the time of crisis. I cannot help feeling, however, that the digestive organs have been occasionally overtaxed by the administration of nutritive food during the fever, and that the crisis is not so complete, or the convalescence so rapid as in the more emaciated cases. When wine has been administered in large quantity it is important it should be diminished when it is evident the poison of the fever has passed away, as shown by the fading of the rash, or by partial crisis, even though a considerable amount of sickness still remains. This



continued illness often arises from secondary disease, which was produced during the progress of the fever, and now continues after its cause has been removed. Stimulants will only help to give this disease a chronic and dangerous character. Mild, easily-digested nutriment will carry the patient safely through this critical period of his illness. With reference to the administration of sedatives the practice of physicians varies very much. Some think a mild opiate, every night, given by the mouth, or in form of enema, useful in tranquilizing the nervous system—of course only in cases where there is no congestion of the brain, active or passive. Others are very unfond of this remedy, as tending to depress the nervous energy, and impair the powers of the digestive organs. As compared with former epidemics the cases at present, in general, may get opium with least risk of injury, as there is seldom cerebral congestion till the end of the disease; but the necessity for it is another question, and must be determined in each individual case. If the patient sleeps in snatches, be it by night or day, as much when taken together as would suffice in health, or nearly so, I would not administer an opiate; what necessity is there that sleep should take place by night rather than by day? or is it not the fact that the first part of the night is that in which the patient is least likely to sleep, as there is generally an exacerbation of his symptoms at that time; and that when nature brings sleep, it is towards morning? There is no doubt we are all liable to fall into the great error of determining, after several sleepless nights, to try an opiate, though we may incline to the belief that cerebral congestion exists, and this on the general supposition that if he does not sleep he must die. But most men's experience will furnish them with cases in which patients have continued without sleep for eight or ten days and nights, and still recovered. It is not the loss of sleep which ever kills, but that state of the brain or other organ which produces the loss of sleep. On the other hand, there is nothing so soothing as an occasional opiate when the want of sleep arises from slight irritation of the brain produced by the febrile state. It will be seen from what I have stated, that our treatment is founded on the belief that the patient is labouring under the effects of a poison which has somehow entered the blood, and has produced changes in the animal economy, which in their progress are governed by laws more or less fixed. That we know absolutely nothing of the intimate nature of this poison but from its effects on the nervous system, the digestive organs, and circulatory apparatus, and that



our treatment should be solely directed to counteract these effects, rather than to endeavour to eliminate a poison of which we neither know its nature, its mode of operation, its manner of entering or departing from the blood. There is no objection to discuss medical theories in books, but it is mischievous to let them influence practice till proved beyond contradiction.

ART. XII.—*Hints on the Diagnosis of Eye Diseases.* By ROBERT BRUDENELL CARTER, F.R.C.S., Stroud, Gloucestershire.

DURING the last few years the labours of ophthalmic surgeons have added considerably to what was previously known about the various imperfections and diseases of the organs of vision; and, as a necessary consequence, have multiplied the means and increased the accuracy of diagnosis. The results thus obtained have hardly yet been brought together in a form sufficiently compendious for ready reference; and, on account of the rapid growth of the subject, it would at present be scarcely possible to give to such a form a character of permanence, or to embody it in a systematic treatise. There are, however, many readers to whom a summary of the existing position of things would be occasionally useful; and hence I have been led to the attempt to frame one, as a contribution to periodical literature.

In order to facilitate description, it is customary to apply to the organs of vision certain technical expressions besides those which relate to their anatomical structure. We take an imaginary line, passing through the apex of the cornea, the centre of the eyeball, and the apex of the posterior hemisphere, and we call this line the *axis of the globe*. The two apices are called respectively the *anterior* and the *posterior pole*. An imaginary line, in a plane perpendicular to the axis, dividing the globe equally into an anterior and a posterior hemisphere, is the *equator*. An imaginary line, in a plane coincident with the axis, dividing the globe equally into a superior and an inferior hemisphere, is the *horizontal meridian*; and a similar line, dividing the globe into a temporal and a nasal hemisphere, is the *vertical meridian*. The same expressions, *mutatis mutandis*, are applied to the crystalline lens. The point to which the eye is directed in the act of vision is called its *fixing point*; and a line drawn from this point to the foramen centrale of the retina is the *axis of vision*. The axis of vision and the axis of the

globe are not quite coincident, but form with one another an angle of about  $6^{\circ}$ , which is usually increased in hypermetropia, and diminished in myopia.

If we except a few disorders of the surface of the eyeball, that is to say, of the conjunctiva and of the peripheral portion of the cornea, we shall find that all other diseases or imperfections of the eye are productive of impairment of function. The patient cannot see perfectly; and it is often from the diminution of his power of vision that he first learns his need of relief.

The degree and nature of this diminution are matters of primary importance to the practitioner, and require to be accurately tested. We investigate the state of vision with regard to its *acuteness*, its *extent*, its *range*, and its *co-ordination*.

The acuteness of vision admits of being expressed in German by the single word *Sehschärfe*, and the letter S., the initial of this word, has come to be used, in other countries also, as a convenient abbreviation of it. In order to determine the value of S. in abnormal conditions, it is first necessary to obtain a standard for health; and such a standard, together with the means of measuring any decadence from it, is furnished by the employment of test-types.

Until recently, the test-types in general use were those of Professor E. Jäger of Vienna; and, although they are now superseded in cases where exactness is required, it is still necessary to be acquainted with them. They are common printer's letters, of twenty different sizes, ranging from what is called "brilliant," (No. 1), to "8-line Roman," (No. 20). A normal eye should read No. 1 at ordinary reading distance; and should read No. 18, and words or letters of No. 16, at a distance of twenty feet.

Professor Donders, when engaged in investigating the phenomena of astigmatism, found the necessity of a more accurate test; and the want was supplied, on the same principle, and nearly at the same time, by Dr. Snellen, of Utrecht, and by Dr. Giraud-Teulon, of Paris. It has long been known that the visibility of an object depends, *cæteris paribus*, upon the visual angle under which it is seen; and Dr. Snellen determined by experiment that "square letters, whose limbs have a width equal to one-fifth of the letter's height are generally distinctly visible at an angle of five minutes." On this basis he constructed a scale of letters, carefully drawn in exact proportion, and containing eighteen varieties. These are numbered consecutively from 1 to 8, and the larger numbers are 10, 12, 15, 20, 30, 40, 50, 70, 100, and 200. They are all so

arranged that the letters are seen under a visual angle of five minutes, when placed at a distance from the spectator equal, in Paris feet, to their own distinguishing number. Thus a normal eye should read No. 1 at one foot; No. 3 at three feet; No. 200 at two hundred feet, and so on.

In using this scale the value of *S.* is found by dividing the distance between the type and the spectator by the number of the smallest letter that can be read with certainty. A person with normal vision will read No. 20 at twenty feet, and therefore, his *S.* will equal  $\frac{20}{20}$ , or unity, which is thus taken as the standard. Another person at the same distance will only be able to read No. 30, and another only No. 100. In the first case  $S = \frac{20}{20} = 1$ ; and, in the second,  $S = \frac{20}{100} = \frac{1}{5}$ ; and the acuteness of vision is respectively only  $\frac{2}{3}$  or  $\frac{1}{5}$ th of what is natural to the eye.

To use the test-types, they should be suspended against a wall in a good light, and the patient placed at a measured distance from them. The smallest letter that he can read with certainty is then found by experiment, and the measured distance divided by the number of the letter. If it be desired to check the trial by another, the letter and distance may be varied. If a patient can read only No. 50 at ten feet, then his  $S = \frac{10}{50} = \frac{1}{5}$ . At twenty feet he should read No. 100; at eight feet he should read No. 40; and at two feet he should read No. 10. The Paris foot, by which the distances should be measured, is three-quarters of an inch longer than the English.<sup>a</sup>

There are cases, however, short of total blindness, in which no letter can be read at all. No. 200 of Snellen's types is a capital A, three inches and three quarters in height; and incapacity to decipher it would amount to incapacity to discern objects. We then can only test the perception of light. If the patient can discern the reflection from a white surface, such as a sheet of writing paper, we say that he possesses *qualitative*, but if he can only discover the direction of the window or the lamp, or can only distinguish between light and darkness, we say that he possesses only *quantitative* perception of light.

By using these tests we are not only able to describe in writing the exact degree of acuteness of vision, but we are able to measure with precision the changes that take place from day to day, and

<sup>a</sup> Jäger's test-types are kept printed, on sheets, by Messrs. Harrison and Sons, printers, St. Martin's-lane, London, W.C. Snellen's may be obtained from any foreign booksellers.

the benefit that has been derived from treatment. For cases of considerable impairment of vision, in which much exactness is not required, the fingers of the surgeon may be used as convenient test objects. Thus we say that the patient can count fingers at two feet, or at three feet, and the distance affords, as it increases, a rough measure of improvement.

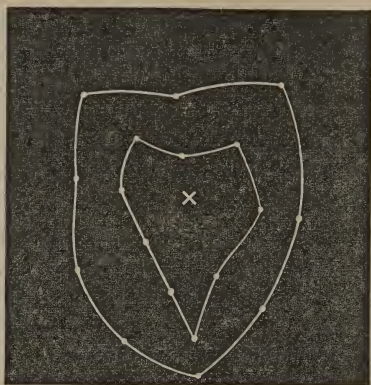
The *extent*, or field, of vision, signifies the extent of surface over which objects can be seen, while the eye remains directed to a central fixing point. It furnishes, therefore, a measure of the functional activity of the peripheral portions of the retina; and its exact determination is of great diagnostic value in many cases.

The field of vision may be roughly tested by the index finger of the surgeon, moved upwards, downwards, to the right, and to the left, in front of the patient, until it passes out of his view in each direction. The eye of the patient must not follow the finger, but must be fixed upon the face or eye of the surgeon; and the finger may be about a foot in front of the patient's face. The eye not under examination must be closed or covered; and, where vision is very imperfect, the finger will be better seen if it be kept moving to and fro on its articulation with its metacarpal bone.

When such an examination as this reveals the need for a more precise one, or when a more precise one is required on account of the nature of the case, the field of vision may be exactly mapped out in the following way:—The patient must be comfortably seated in front of a black board, about a yard square, having its centre level with, and opposite to, the eye under examination, and eight inches distant from it. This distance may be maintained by some support for the head. At the centre of the board a mark is made with chalk, and the patient is directed to look at it steadily. The eye not under examination is covered by the hand. The surgeon then takes a piece of chalk, fixed to the end of a black rod, and, commencing from one side of the board, moves the chalk towards the centre until it becomes visible to the patient. At the point where it becomes visible a mark is made, and the chalk is moved onwards until it becomes *clearly* visible, when a second mark is made. The chalk is thus drawn from the circumference to the centre in several directions, as many as may be required; and then a line is drawn to unite the outer or first series of marks, and another to unite the inner or second series. The latter line will include the field of clear vision; between the two lines will be a zone in which vision exists, but is indistinct; and the first line



marks the outside boundary of the field. The annexed diagram shows the kind of map that may be obtained in the manner described; and it will be observed that the fixing point  $\times$ , although central on the board, need not be central in the field of vision. With proper care, the limits of the natural blind spot, situated on the temporal side of the fixing point, and corresponding to the surface of the optic disc, may be accurately traced; and sometimes morbid blind spots will be found in other



portions of the field. These always require to be most carefully examined and defined. It is well to have the board divided into squares by slightly grooved lines, and to keep diagrams drawn in the same proportion on a reduced scale, on which a field of vision may be copied from the board, and preserved in a convenient form for future reference.

When very great accuracy is required, it is well to test the first map by a second, obtained by moving the chalk in a direction from the centre to the circumference. It is also often necessary to repeat the examination several times, in different degrees of illumination, in order to discover the sensitiveness of the retina under different amounts of light. The letters *Se.*, that is, *eccentric Sehschärfe*, are used as an abbreviation to denote peripheral vision.

The *range* of vision refers to distance only, and is included between the far point, *r*, *punctum remotum*, and the near point, *p*., *punctum proximum*. The former signifies the farthest, and the latter the nearest point at which objects can be clearly seen. In normal eyes, there is really no far point, no distant limit of vision, and objects only cease to be visible when so far removed that they no longer yield sufficient light to stimulate the retina. For every healthy eye, however, there is a distinct near point, within which fine print is no longer legible; and this near point naturally becomes more remote as life advances. Its distance is the measure of the power of the eye to *accommodate*, that is, to increase its refraction by volitional effort; and this power is chiefly due to an increase in the curvature of the anterior surface of the crystalline lens, effected by the action of the ciliary muscle. With the lapse

of years, the lens becomes harder than in youth, and its shape is less easily modified; while the muscle itself must sometimes become less vigorous. In most people, therefore, the near point becomes inconveniently remote prior to old age; so that a newspaper has to be held at arm's length. This condition is called presbyopia. Within certain limits, it is strictly physiological; and it is arbitrarily defined to commence when the near point is ten inches from the eye.

When the far point is at some finite distance from the eye, that is to say, when vision becomes indistinct as the object is removed *beyond* some definite distance, as one foot, or six feet, we have the condition called myopia, or shortness of sight. The place of the near point can always be measured by moving the object of vision to and fro on a sliding scale, while the eye is kept stationary; but, in order to determine the far point in this way, a scale of inconvenient length would be required. It is shortened by making the patient look through a convex lens, which brings the far point of a normal eye to the principal focus of the lens, and the far point of a myopic eye *within* this focus. If the distance of the far point, as seen through the lens, be made into a fraction by placing 1 as its numerator, and if the focal distance of the lens be made into a similar fraction, and if the lesser of these fractions be subtracted from the greater, the result will be another fraction, the denominator of which will express the actual distance of the far point. If the lens chosen have a focal distance of six inches, and if the far point of a person looking through it be five inches, then  $\frac{1}{5} - \frac{1}{6} = \frac{1}{30}$ , and the actual far point will be at thirty inches. If the lens be of ten inch focal length, and the far point through it be at nine inches, then  $\frac{1}{9} - \frac{1}{10} = \frac{1}{90}$ , and the actual far point will be at ninety inches. In these experiments the distance of the object should not be measured from the eye of the patient, but from the centre of the convex lens.

The *co-ordination* of vision refers, of course, to the harmonious union of the two eyes in the visual act. When this union is disturbed, the first result is that the images formed upon the two retinae are no longer blended, and diplopia, or double vision, is produced. It is then necessary to determine whether the images are crossed or not—that is to say, whether the right hand image belongs to the right eye or to the left. For this purpose the flame of a taper is chosen as an object; and the patient holds a slip of red glass before his best eye, while he looks at the flame with both. The reddened image, whether it be on the right or the left, will belong, of course, to the eye before which the glass is held. If the

images are crossed the eye in fault is deflected outwards, and if they are not crossed the eye in fault is deflected inwards. The extent of the deflection may be estimated by the distance apart of the images; and it may be sufficient to cause very troublesome diplopia without amounting to a visible squint. The most frequent cause is a partial paralysis of either the internal or external rectus muscle, arising from some affection, centric or eccentric, of its motor nerve.

Sometimes it happens that double vision ceases, and yet that proper binocular vision is not restored. When this is so the patient only uses one eye, and suppresses the image received by the other. If the eye be tolerably sound, and the image of normal brightness, the latter can seldom be suppressed without a decided squint, which causes it to fall upon the peripheral and less sensitive portion of the retina. If the retina be from any cause torpid, the second image may be suppressed without a squint.

The presence of binocular vision is tested by placing before one eye a prism, with its angle to the nasal side, while the patient looks with both eyes at a candle flame eight or ten feet distant. If binocular vision be perfect the patient will either see two images, or the eye furnished with a prism will squint inwards to overcome its refraction. If neither result follow we conclude that the patient sees habitually with one eye only. Another and very simple test is to let the patient read, at his ordinary reading distance, and to hold a pencil, or other small object, half way between his eyes and the book. With proper binocular vision he will see two images of the pencil, neither of which will interfere with his reading. With monocular vision he will see only one image of the pencil, and this will shut out a part of the page from his view. It must be remembered, of course, that monocular vision does not imply blindness of either eye, but simply loss of the co-ordination between the two.

Next to impairment of vision the most prominent symptoms in eye diseases are pain and congestion.

The pain may be either referred to the globe itself, in which case it is most frequently produced by heightened tension, or it is felt chiefly in the parts around the orbit, and especially in the filaments of the supra-orbital branch of the tri-facial nerve.

The congestion presents itself under three principal forms. In the first the vessels that perforate the sclerotic near the cornea, and pass backwards towards the equator, are distended and tortuous; in the second the cornea is surrounded by a delicate zone of redness; and in the third the general surface of the conjunctiva of the globe



is covered by a network of vessels. The second of these conditions is sometimes concealed by the presence of the last; but they are all too well known to require any detailed description.

We will now suppose that a patient presents himself suffering from considerable bright redness of the conjunctiva of the globe of one or both eyes, and complaining of some smarting and discharge. We place him with his face towards a good light, and observe—first, that the cornea retains its natural polish and transparency; secondly, that the aqueous humour is clear; and, thirdly, that the pupil is freely movable.

The polish of the cornea is best tested by its action as a reflector. In the healthy state it reflects a perfect image of any object placed in front of it, such as a candle flame, or the sashes of a window, or the face of the spectator; and if this image be in any way blurred, broken, or distorted, we have to deal with some disturbance of the corneal epithelium, or some departure from the symmetry of the corneal surface.

The transparency of the cornea and the aqueous humour is best shown by the clear visibility of the peculiar fibrous structure of the iris, and by its natural colour. Sometimes, when the cornea is transparent, the aqueous humour will be yellow and turbid, so that the iris will be only indistinctly seen, and, if naturally blue, will appear of a greenish tint. In such a case a puncture of the cornea, by allowing the escape of the turbid and discoloured fluid, will restore the natural appearance of the iris.

Supposing, however, that the cornea is bright and the aqueous humour limpid, attention should next be directed to the pupil. The eye not under inspection being closed, the shadow of the surgeon's hand should be allowed to fall upon the other. The pupil, if healthy, will instantly dilate, and will contract again when the shadow of the hand is removed. If it does so we have only to find that the natural acuteness of vision is unimpaired, in order to arrive at the conclusion that the disorder is external to the eyeball, and strictly limited to the conjunctiva. If the acuteness of vision were diminished we should infer that the choroid was congested and that there was danger of the external inflammation being complicated by some form of choroiditis.

In applying this test to a stranger, we must not hastily conclude that he possessed  $S=1$  prior to the attack of conjunctivitis. It is very common to find the acuteness of vision below the natural standard in persons who are quite unaware of their own deficiency in this respect, and who suppose that they see as well as their



neighbours. In any case of apparently simple conjunctivitis, with S. below unity, it is well to test the sight by some familiar object, and to make sure that vision is not only defective, but recently impaired, before suspecting choroidal complications.

There are several forms of disorder that implicate the cornea, and that do not affect vision unless they encroach upon the portion in front of the pupil, or unless they are attended by excessive photophobia. These disorders are mostly vesicles, pustules, ulcers, or various inflammatory affections, presenting many points of analogy to certain diseases affecting the skin; and their characters, as well as those of the varieties of purulent ophthalmia, are usually to be read at a glance. Modern researches have done little more than to clear up their precise anatomical and pathological characteristics and analogies; matters of great interest, but with which we have here little to do. It is necessary to remember that a corneal ulcer, especially when beginning to heal, is often surrounded by a considerable zone of cloudiness; and hence that this cloudiness, although the ulcer itself be quite eccentric, may encroach upon the pupillary space, and seriously interfere with vision.

There is, moreover, an affection principally of the cornea, although often implicating the deeper structures of the eye, which is attended with impairment of vision from its very commencement, and which has been placed in an entirely new light by the genius and the labours of Mr. Hutchinson. This is the corneal inflammation peculiar to persons who have inherited the syphilitic constitution (or who, as I believe, have acquired it by vaccination); and its detection is of the highest importance, because the treatment it requires is of a special character. It is recognized in mild cases by a peculiar aspect of the cornea—an aspect that usually commences as a slight central haze, that becomes subsequently mottled or dotted, and eventually passes into an interstitial cloudiness. It is attended by a comparatively small amount of congestion, is prone to attack first one eye and then the other, and is associated with the peculiar facial conformation, the earthy complexion, the scarred angles of the mouth, and the notched, crescentic, or peggy teeth that are so graphically described in Mr. Hutchinson's monograph.

When the surgeon is satisfied, by the tests already mentioned, that he has to deal with an inflammation that is external to the eye-ball, his next point is to ascertain whether there is any external cause to account for it. For want of this simple precaution I have several times seen patients who had been energetically treated, by

local and constitutional means, for symptoms entirely due to the presence of a foreign body. The cornea should, therefore, be carefully scrutinized, to see that nothing is impacted in its substance; the growth and direction of the eyelashes should be noted; the upper eyelid should be everted and the lower drawn down, and the openings of the canaliculi inspected with the aid of a lens, in order to see that no fallen eyelash has become entangled in them.

Patients sometimes present themselves, however, in whom redness of the conjunctiva is associated with impairment of vision and with turbidity of the aqueous humour, so that the fibrous structure and proper colour of the iris are somewhat disguised. There is then acute congestion or actual inflammation of the choroid and iris. The extent to which the iris is inflamed may be seen by the number and size of the adhesions between the margin of the pupil and the anterior capsule of the lens; and these adhesions, if not readily discernible when the pupil is exposed alternately to light and shade, may be rendered manifest in a few minutes by the instillation of a drop of a solution of atropia containing four grains to an ounce of water. The surgeon will arrive at a conclusion as to how much impairment of vision is due to the turbidity of the aqueous humour, to the partial closure of the pupil, and to the effusion of lymph; and, if the actual impairment be much in excess of what can be thus accounted for, he will conclude that the choroid participates in the disease. When it does so its participation will usually produce increased tension of the globe.

In order to estimate *tension* the surgeon should familiarize himself thoroughly with the peculiar resilience of the healthy eye. The method of examination is to make the patient gently close his lids, as if in sleep, while the surgeon places the tips of both forefingers upon the upper part of the globe a little apart, and by alternate gentle pressure, feels and estimates the degree of resistance. Mr. Bowman makes nine degrees of tension, four above and four below the normal, and expresses them by a scale of T's, as follows:

$$-T^3 - T^2 - T^1 - T^0 \quad T^0 + T^1 + T^2 + T^3.$$

This scale is easily intelligible. In  $-T^0$  and  $+T^0$  the departure from the normal standard is doubtful; in  $-T^1$  and  $+T^1$  it is no longer doubtful; in  $-T^3$  the globe is perfectly soft; and in  $+T^3$  it is as hard as a stone. The only difficulty about the T's is the impossibility of referring them to any common standard. The  $T^3$  of one surgeon might be the  $T^2$  of another.

Disregarding this unavoidable difficulty, if a patient presents

himself in whom there is manifest iritis, with greater diminution of vision than the iritis present will account for, and with evident but not extreme increase of tension, we infer that the choroid is seriously implicated in the disease.

Such cases must not be confounded with others, happily rare, and, still more happily, remediable by prompt treatment, in which the globe is as hard as stone, the conjunctiva injected, the cornea steamy, the pupil dilated and fixed, the pain intense, and vision almost abolished. This is *acute glaucoma*, and requires the immediate performance of a large iridectomy, in order to save the eye from speedy and complete destruction.

In order accurately to estimate the nature and the significance of tension, or in other words, of abnormal fulness of the globe, it is necessary to remember that it may arise either from the active congestion and the effusions due to acute inflammation, or in a manner more insidious, by a disturbance of the natural balance between secretion and absorption within the eye. The former condition, usually due to irido-choroiditis, is most frequently seen in the eyes of persons not past middle age, in whom the sclerotic coat retains a degree of elasticity that permits it to yield somewhat to the pressure from within. Partly from this yielding, and partly because inflammatory tension is seldom excessive in degree, it is not, as a rule, immediately destructive to the retina; but it imparts to the inflammation a very intractable character, so that if the tension be not relieved, the eye will usually be lost eventually; and, on this account, its early recognition is of the highest importance.

The more chronic form of tension that is produced by disturbance of the balance between secretion and absorption, is mostly seen in the eyes of persons past middle age, in whom the elasticity of the sclerotic is much impaired. It is the cause of the several varieties of glaucoma; and the precise character of the phenomena, in any case, will be chiefly due to the rate at which the tension increases, and to the degree of resistance that is opposed to it. The continued existence of excessive intraocular tension necessarily produces compression of the retina, and consequent loss of sight. The whole surface of the retina suffers, but the peripheral portions suffer earlier than the central. Consequently, the field of vision becomes gradually contracted; and, although central vision is always impaired in some degree, it may remain comparatively good up to a late period of the disease. In the end, however, unless the tension be relieved, total blindness will be the result.



For some reason, either from weakness of the ciliary muscle by interference with its supply of blood, or from pressure upon its motor nerves, or from the production of a premature hardness of the crystalline lens, the early stages of increased tension are usually attended by rapidly increasing presbyopia. Where this symptom presents itself, a careful examination of the degree of hardness of the globe should never be omitted.

The ocular tissues, like other textures of the body, show a marked tolerance of pressure that is only very gradually increased; and a decided intolerance of the same degree of pressure when suddenly or quickly produced. While, therefore, in cases of very gradually increasing tension, the only symptoms may be presbyopia, dimness of sight, contraction of the field of vision, cupping of the optic disc, and eventual blindness, it is more common to meet with patients in whom a more rapid rate of increase is productive of more or less inflammatory reaction. In such cases the dimness of sight is liable to be increased suddenly for short periods, which may be followed by partial improvement; and it is common for the exacerbations to be attended by ciliary and orbital neuralgia, by the appearance of coloured fringes around a flame, by congestion and inflammation of the ocular tissues, and by opacities in the crystalline lens and vitreous body. When the increase of tension is very rapid the symptoms are correspondingly severe.

The more typical examples of these three gradations of rapidity are called respectively, simple chronic glaucoma, sub-acute glaucoma, and acute glaucoma. They possess common characters, depending upon exalted tension, and differential characters, depending upon the rate at which that tension has increased, and upon the amount of resistance opposed to it by the sclerotic. In all of them the tension is the first demonstrable link in the chain of morbid action; and must be carefully distinguished from the tension that arises as a mere symptom of choroiditis. The history of the disease will usually clear up all doubt; and the important difference must be remembered, that the tension which produces glaucoma tends to steady increase; while that which arises in the course of choroiditis would disappear with the disappearance of its cause, and may, therefore, sometimes yield without calling for any special treatment.

In cases where the surgeon decides that he is dealing with a disease that is primarily iritis, or irido-choroiditis, the question will often arise:—What is the character of the inflammation? Is it common or specific?



The forms of specific inflammation chiefly noticeable are those dependent upon syphilis and upon gout. I am not aware of any pathognomonic sign by which either of these forms may be recognized, and I am quite sure that many of the signs mentioned by authors are occasionally deceptive. If the frequent occurrence of these forms of inflammation be kept before the mind, if the state of the patient be carefully examined, if his history be carefully inquired into, and if tentative treatment be cautiously employed in doubtful cases, there will be little liability to any error of importance.

We may now pass on to the consideration of cases in which there is defective vision, without any striking change in the external aspect of the eye. There may be trifling congestion produced by straining efforts to see, but no intense redness, no ulcer or opacity of the cornea, no turbidity of the aqueous humour, and no visible change in the iris.

In such a case we have first to inquire whether there be any error of refraction, that is, whether the patient be myopic or hypermetropic. To determine this point the test types are used, and the patient is tried first with low-powered concave and then with low-powered convex glasses. If the concaves improve vision a succession of trials will determine what powers raise the vision to the highest attainable point. If the convex lenses are beneficial a similar trial cannot be accurately made until the accommodation is paralysed by atropine. For this purpose it is necessary to use a drop of the four-grain solution, and to allow two hours for its action.

In cases where either concave or convex glasses raise S. nearly to the natural standard we may usually be content; but if they produce a comparatively small amount of benefit we may suspect the existence of astigmatism.

Astigmatism sometimes depends upon irregular formation of the crystalline lens; but its usual cause is an excess of the natural difference between the vertical and the horizontal curvatures of the cornea. Its chief characteristics are—the inability of the patient to perceive vertical and horizontal lines at the same time with equal distinctness—and the improvement of vision that is produced by looking through a narrow slit in a blackened card or a plate of metal, when this slit is placed in some particular direction, to be found in every case by trial. The estimation of the precise kind and degree of astigmatism, so as to be able to prescribe the nature and position of the glasses calculated to relieve it, is too intricate a

matter to be here described. Full information may be found concerning it in the great work of Professor Donders, and sufficient information for most purposes in the smaller treatises of Mr. J. Z. Laurence and Mr. Soelberg Wells.

There is another form of corneal deformity that seriously impairs vision, namely, conicity. This condition, when developed, is too conspicuous to be overlooked; and, even in its early stages, it is usually very manifest when the eye is examined in profile.

If there be no evidence of the presence of astigmatism, or of any other irregularity of the cornea, the surgeon may next direct his attention to the crystalline lens, with especial reference to its transparency.

In order to examine this structure two methods are available—focal illumination and the use of the ophthalmoscope. For young people the former, and for old people the latter, will furnish the most uniformly satisfactory results; although it is always desirable to employ both. To use focal illumination a lamp is placed at the side of the patient's head, and somewhat in front of it; or a beam of daylight passing through a small hole in a screen may be used instead. However obtained, the light is concentrated by a bi-convex lens upon the part of the eye under examination, and the portion thus illuminated may be examined through another glass used as a magnifier. The management of the cone of light will need a little dexterity that only some practice can give; and in order to examine the lens the pupil must be completely dilated by atropine.

In the eyes of young people, who suffer from defective vision, we may find, at least, two forms of cataract, the first consisting of a filmy, or dotted, or stippled opacity, extending to the equator of the lens, and implicating the external cortical substance in some degree. In the second form the opacity is central, sharply defined, and surrounded by a zone of perfectly transparent tissue in which no trace of cloudiness can be discovered. The difference between the two forms is very important; since in the first the opacity will spread steadily over the whole lens, and in time produce blindness. In the second the opacity has little or no tendency to extend itself, even though it may become more dense. The transparent zone will usually remain transparent, at least for many years; and, if it be wide enough, useful vision may be restored by the employment of atropine, or by displacement of the pupil without removal of the lens.

Among the more rare conditions that focal illumination may

reveal must be mentioned dislocation of the lens, which may occur in almost any direction. Such displacements are usually found in both eyes, and in the same direction in both with regard to the median line. The displaced lenses are often not perfectly transparent.

The faint opacities of the lens that may be present in young people, and seriously affect vision, are readily seen under focal illumination by their greyish or whitish colour; but they may sometimes be so generally diffused as not to be very evident when looked for with the ophthalmoscope. The commencing cataract of the aged, however, which consists of densely opaque striæ not always differing greatly in colour from the rest of the lens, is most readily detected by the ophthalmoscope. The striæ appear as densely black lines on an illuminated ground, and can be recognized at a single glance with unfailing certainty. The only appearances liable to be mistaken for them are spots left on the anterior capsule by former iritic adhesions; and these, instead of being radiating lines, are disposed in a figure more or less circular.

When senile cataract is fully developed, and the amber-coloured nucleus is surrounded by a layer of milky or spermaceti-like cortical substance, an erroneous diagnosis is scarcely possible. But there are certain states of the eye, in elderly people, in whom the blackness of the pupil is lost, that are frequently mistaken for cataract. The pupil often presents a peculiar appearance of yellow cloudiness due to reflection from the fundus, and very likely, when seen by diffuse daylight under atropine, to deceive a hasty or an inexperienced observer. The distinctive character of this cloudiness is its uniformity. In focal illumination, in such a case, the pencil of light passes over the crystalline without a break; and this it never does when there is cataract. The ophthalmoscope will also afford conclusive evidence, by failing to show the opaque striæ mentioned in the preceding paragraph.

When the above methods of examination display a transparent condition of the lens, the cause of impaired vision may next be sought in turbidity of the vitreous body, or in the various diseases of the retina, optic nerve, or choroid, recognizable with the ophthalmoscope. For a full account of all these conditions, of the methods of discovering them, and for suggestions with regard to their treatment, I must refer the reader to my translation of Zander on the ophthalmoscope. The subject is far too large to be contained within the limits of this paper; and no advantage could accrue from a very imperfect sketch of it.

There are however cases, still very obscure, in which vision is impaired or destroyed, and in which the ophthalmoscope affords but little information. By its aid, in such cases, we may often discover certain atrophic changes in the optic nerve; but the symptoms of impaired vision commonly precede the occurrence, or at least the recognition of these changes, and the atrophy may be an effect rather than a cause. Subsequent symptoms, or death, may occasionally reveal a source of blindness seated deeply in the nervous centres; and symptoms referable to the nervous centres may sometimes precede or accompany the first failure of vision. Still, we have not always even so much light as this; and, when we find defective vision that is not due to any visible change, inflammatory or otherwise, in the transparent media or the ocular membranes, or to any obvious mechanical interference, as by embolia, with the internal ocular circulation, we have no resource but to fall back upon the nomenclature of the pre-ophthalmoscopic period, and to describe the defect, according to its degree, as amblyopia or amaurosis.

It is evident that such cases, by reason of their obscurity, as well as by reason of the importance of their issue to the patient, require the most careful possible attention; and that the question of their probable termination must, at a very early period, force itself upon the mind of the practitioner. They have lately been made the subject of clinical lectures by Professor v. Graefe, from whose remarks upon them the following statements are derived.

According to v. Graefe, our opinion about a case of amblyopia or amaurosis should rest mainly upon three elements, namely, an exact investigation of the visual function, the appearance of the optic disc, and the manner of development of the disorder.

In the investigation of the visual function the degree of central vision is highly important; but, as regards prognosis, it is still more important to test carefully the limits of the field, and the state of of eccentric vision. Experience has fully shown that cases which tend to the progressive extinction of sight are distinguished by contraction of the field, or by obscurity at its limits; and it is evident, *a priori*, that an increasing atrophy of the nervous elements of the retina would be first apparent in the parts most remote from the supply of blood.

For such cases the ordinary method of taking a field of vision is not of sufficient delicacy, and must be refined by the addition of a means of gradually decreasing the illumination. The piece of chalk may also be moved outwards, from the fixing point, as well as



inwards from the periphery. The results of the examination may be arranged under three heads:—

1. The peripheral extent and acuteness of vision are comparatively identical with those of a sound eye—*absolutely normal*.

2. There is diminution of the acuteness of peripheral vision, alike in all directions, and very small in degree relatively to the impairment of central vision—*relatively normal*.

3. The impairment of eccentric vision is irregular—*i.e.*, exists in only one part of the field, or in one direction, or preponderates in certain directions, and is disproportionate in these to the central impairment—*abnormal*.

With absolutely normal eccentric vision we may exclude the fear of progressive atrophy in any case in which the disease has already made some progress. There may be a nascent stage of atrophy in which eccentric vision is not affected; and it is therefore necessary to form a guarded prognosis in cases of recent and undeveloped amblyopia. If the impairment of vision be only of a few weeks' duration, and not below two-thirds of the normal, and if the peripheral vision be absolutely normal, it is still possible that with greater diminution of vision the periphery may begin to suffer in a greater degree. But if the case be of several months standing, with S. depressed to one-sixth, or one-tenth, or less, and if the peripheral vision be then absolutely normal, we may say with certainty that we have not to deal with a case of progressive atrophy. The apparently worse condition will justify the more favourable prognosis, in so far as regards the prospect of eventual amaurosis, but without reference to probable recovery. For we find a number of cases of amblyopia, with unaltered peripheral field of vision, but with central or eccentric scotomata, which resist all treatment, and remain stationary at a certain point.

When the field of vision is relatively normal, this circumstance alone leads to no conclusion; and is only significant when considered together with the aspect of the optic nerve, and the duration and manner of development of the disease. The condition is such as may be produced by the exclusion of one eye from the visual act, as in strabismus; and it may also occur in cases in which, the cause of the disease being in continued operation, as in amblyopia potatorum, an eventual progressive atrophy may ensue. But, when the field of vision is relatively normal, a progressive atrophy is not actually present in a characteristic form; and the case should be hopefully submitted to a rational and individual treatment.

If the field of vision be abnormal, an unfortunate issue is to be expected; but it would be going too far to say that it must necessarily ensue. We shall be guided to a right prognosis by the nature of the limitation of the field, by the relation of the eccentric to the central vision, by the aspect of the optic disc, and by the manner of development of the disease.

If the field of vision in both eyes be curtailed on the same side of the body—as to the right side, or the right upper, or right lower portion—with the central vision nearly or quite normal, the mischief is limited to one optic tract, and may proceed to complete hemiopia, but not to complete blindness. There are some very rare cases, which have as yet received no anatomical explanation, in which there is a defect in the upper or the lower portion of the field, in one or both eyes, the defects being in the latter case symmetrical; and in which, when the defect is sharply defined against normal retina, and when the central vision is normally acute, there is no tendency to progressive atrophy. Concentric narrowing of the visual field by successive diminutions without change of form, as in some cases of glaucoma, may sometimes go on to blindness; but more frequently will admit of arrest or of recovery. Recovery may be looked for with some confidence when the optic disc is of normal aspect, when vision is only moderately impaired, and when the field is enlarged by the use of dark blue spectacles, as in some cases of hysteric anesthesia retinae, and in some irritable children. The most unfavourable prognosis must be formed in cases of irregular lateral narrowing of the field, occurring simultaneously or successively in both eyes, and in such a manner that the field is most affected in both in the same direction, as towards the inner or towards the outer side, or in some intermediate situation, outer and lower, or inner and upward, as the case may be. The expression “most affected” suggests what is usually the case, that all parts of the field suffer in some degree. Such cases are distinguished from those of hemiopia already referred to, by the circumstance that the affected portions of the field are never sharply defined from other portions that retain their normal function. The common course of amaurosis is for one eye to be already nearly blind, or with its field of vision contracted nearly to the fixing point, before the other begins to suffer. In order to establish the soundness of the second eye it is necessary to test its vision with great care, and to find it perfect, especially in the particular direction in which the first eye began to fail. Such examinations are highly important, because,

for reasons at present unexplained, we sometimes see an interval of a year or two, or even of several years, between the failure of the two eyes; while, in other cases, the one follows the other rapidly. Most commonly, moreover, amaurosis first shows itself on the nasal side of the periphery; a fact possibly to be explained by the anatomical distribution of the fibres of the optic tract. The reverse obtains too frequently to be regarded as a mere exception, but only as a less usual occurrence. When the second eye begins to suffer in a direction not homologous with the point of commencement in the first, not both on the medial, or both on the temporal side, but on the same side with reference to the halves of the body, right or left, there may possibly be only a disease of one optic tract, showing itself in succession in the parts supplied by the lateral and by the cruciate fibres, and tending not to blindness, but only to hemiopia. This hope may be entertained if the failure of sight in the first eye does not transgress a vertical line drawn through the fixing point; and if the vision be not less than from one-third to one-fourth of the normal. Under other circumstances progressive atrophy is to be dreaded.

While the investigation of the periphery of the field of vision is thus of great importance, the investigation of its continuity is scarcely less so, especially with regard to the prospect of recovery. In a general way those forms of amblyopia present the greatest likelihood of complete recovery in which the conducting power of the nervous filaments has the smallest deviation from the normal. If in a given diminution of S. there is also a completely proportional state of Se., so that this, after the common rule, diminishes continually from centre to periphery, without interruptions or sudden diminutions at any point, we augur more favourably, *quoad* recovery, than when the diminution of central vision is itself circumscribed and sharply defined against the surrounding parts (central cloud, central scotoma, central defect), or than when, even with less lowering of S., there are any eccentric interruptions. In the cases of central or eccentric scotoma the causes in operation seem often to produce permanent destruction of the conducting elements. At least we often see such defects remain unchanged, and hence our prognosis about them, although favourable, *quoad* blindness, is very doubtful, *quoad* recovery. A sound judgment upon the point may often be obtained by attention to the aspect of the optic disc, and to the mode of development of the disease. It is well to remark, however, that in order to justify a

favourable prognosis, *quoad* blindness, either in central or eccentric scotoma, it is imperatively necessary that there should be satisfactory Se. beyond the defect. If we find a scotoma beyond which, towards the periphery, either generally or in certain directions, the Se. is reduced, the case will, as a rule, terminate in progressive atrophy. Scotomata placed symmetrically in both eyes call for very careful examination in this respect. If the scotomata be in the lower part of each field, and if beyond them, that is below them, the Se. is completely normal, the function of the conducting fibres corresponding to the defect must be complete, and blindness is not to be feared. Otherwise we must anticipate amaurosis.

The intensity of the illumination used is a matter that requires attention. In the normal eye we find that a very considerable diminution of light, from full day-light downwards, scarcely at all affects the clearness or extent of peripheral vision; while in many morbid conditions a very slight diminution is productive of great decrease of visual power, indicating a very general torpor of the perceptive elements of the retina.

We proceed now to consider the appearances of the optic disc in cases of amaurosis; and we have chiefly to observe it with reference to four points, namely—colour, opacity, excavation, and dwindling of the vessels.

The *colour* of the normal optic disc, however much it may incline to white or yellow, is always modified by a stippling or tinting of red, and only here and there, as within a physiological excavation, displays the clear bluish white reflex of the lamina cribrosa. In many forms of amblyopia, and especially in the more threatening, the whole of the papilla loses its natural redness, and becomes more or less uniformly white, thus contrasting strongly with the surrounding choroid, and appearing more sharply defined than usual. The causes of this appearance are twofold—partly that with the wasting of the nerve elements the lamina cribrosa becomes more conspicuous, partly that some thickening of the connecting tissue of the nerve has taken place. These two causes may be either separate or conjoined in their operation. If the first alone be concerned, we see an atrophic excavation presenting a bluish white colouring; if the second, an intensely white disc with a flat surface; if both, a superficial excavation, with the details of the lamina cribrosa clearly visible in the position of a pre-existing physiological excavation, but covered elsewhere by white connecting tissue. The preponderance of one or other form seems to depend,



not on any difference in the atrophic process, but on the precise formation of the papilla, on the character of the intra-ocular pressure, and on the state of the circulation in the connecting tissue. The second test, the *opacity*, may be very briefly dismissed. The delicate semi-transparence of the healthy disc is of necessity done away with when the visible surface is formed either by the lamina cribrosa or by the thickened connecting tissue in front of it. It is then no longer possible to see the vessels sinking into the nerve tissue. The presence or absence of *excavation* depends upon the condition of the connecting tissue at the end of the nerve. Concerning the *diminution of the vessels*, it will sometimes happen that they all, including the larger trunks, become reduced in calibre; but, for the latter, this is not an invariable occurrence. We even see cases of old and complete amaurosis, in which the optic nerve exhibits all the marks of complete nervous atrophy, but in which the larger vessels retain their normal diameter. The case is wholly different when atrophy of the nerve is a result of certain intra-ocular processes, such as choroido-retinitis; for then the diminution of the large vessels is of invariable occurrence. The cause of the difference seems to be that in amaurosis, as in division of the optic nerve, only the fibrous and ganglionic layers of the retina dwindle, while the others retain their structure. In choroido-retinitis, on the other hand, all the elements of the retina perish. The different conditions of the main vessels, seen in atrophic amaurosis, probably depend upon different relations to the connecting tissue. The finer ramifications upon the papilla are, however, always either diminished or absent. The reddish tint of the healthy papilla, doubtless, depends upon a multitude of fine vascular twigs inter-penetrating its substance; and the want of these materially contributes to the bleaching of its surface. It appears that the very fine twigs are mainly subservient to the nutrition of the intra-ocular extremity of the nerve, and of the fibrous bundles surrounding it; and that these twigs share in the wasting of the fibrous layer, while the greater part of the retinal vessels are the channels of nutrition to the remaining retinal layers. The pallor of the disc, produced by diminution of the fine twigs, is necessarily an important element in the production of colour changes; and, while the natural semi-transparence is retained, this pallor may precede all other changes; and may thus mark an initial period of the disease, in which coarse methods of testing the visual field afford, as yet, no result, and in which the most accurate examination is required in order to reveal

the torpor of the periphery that is due to slight functional impairment of the fibrous layer.

The progress of development of all the above-mentioned conditions is briefly described as atrophic degeneration of the optic disc. It must be remembered that bare inspection cannot reveal, in any case, whether the atrophy be stationary or progressive; and that upon this depends its significance. An entire absence of atrophic change, however encouraging under certain circumstances, does not, under others, exclude the most unfavourable prognosis; while the presence of atrophy, to an evident degree, may sometimes depend upon a local error of nutrition, and may not necessarily lead to blindness.

The third important point on which our judgment must rest, is the mode of development of the disease and of the attendant symptoms.

We will first consider the occurrence of sudden or rapidly developed disturbance of vision—in the form of limitations of the field, sharply defined, and either hemiopic or concentric—of central scotomata—or of total blindness. It was formerly believed that all suddenly occurring pathological conditions were results of hemorrhagic effusion; but this belief is now greatly modified; and it is known that the causes of such conditions are not only various, but often very obscure. Even when impairment of vision follows hematemesis, or gastric derangement, or some exanthem, the connexion between antecedent and consequent is at present unexplained. In suddenly occurring cases we must usually be content to wait and observe before forming a prognosis; but, as a general rule, this may be more favourable in children than in adults; and even the total absence of perception of light does not preclude all hope of recovery, although it may continue for one or more weeks. Its longer duration, especially when associated with atrophic degeneration of the nerve, must be regarded as very unfavourable.

The gradually occurring cases are by very much the most frequent. As a rule, in cases originally favourable, the disorder occurs in both eyes at once. As regards our prognosis, *quoad* blindness, we are greatly helped when the disease remains for some time stationary. The worst forms make, indeed, occasional pauses; but these pauses seldom last longer than a month or two. A long period of arrest in the same condition, with a normal aspect of the nerve, and with no contraction or interruption of the field of vision, will permit a hope of recovery; since such cases are often successfully treated by careful attention to their constitutional causes.

In cases of contraction of the field of vision the method of development is of great importance. Hemipic contractions, when not occasioned by apoplexy or encephalitis, sometimes depend upon some idiopathic and transitory change in one optic tract. Such changes may be effects of syphilis, or of obscure nervous conditions, which the progress of the disease may not always clear up. In such favourable cases, the development of the disease is usually rapid; and the acme is reached in the course of a few weeks. Sometimes, but very rarely, we observe temporal hemiopia with rather quick development, and we may conclude, from the complete recovery of such cases, that some transitory affection of the cruciate fasciculi of the optic nerve has occasioned them. I will not affirm that it is always practicable to distinguish at first these forms from those fatal to vision; but we may take as guides the rapid, almost simultaneous, and symmetrical development of the disease in both eyes—the relatively good central vision, not reduced below one-fourth or one-sixth—and that there is still complete integrity of the optic disc, after some weeks have already passed away. When one eye becomes completely blind in a comparatively short time, and the other, some months afterwards, remains completely sound, the ultimate prognosis for the latter is far more favourable than if the disease in the first eye had progressed slowly, and had rendered it only partly blind. In the first case there would probably be atrophy of one optic nerve in front of the chiasma, since it is highly improbable that the lateral fasciculus of one side, and the cruciate fasciculus of the other, should both be diseased without the participation of neighbouring fibres.

When considerable impairment of vision follows acute cerebral symptoms, we must not be too hasty to give an unfavourable opinion; since in such cases improvement may take place, and a certain amount of sight is often retained. We even see cases of complete blindness from acute affections of the brain, in which, after several weeks, or, exceptionally, several months of complete absence of all perception of light, some vision will return. We must be careful however, not to hold out delusive or groundless hopes; and, when long-continued blindness is associated with degeneration of the papilla, it is obvious that the chance of improvement is only slight, and can at best refer only to a small part of the visual field. But in the slowly advancing cases without marked brain symptoms, no improvement is possible, and only temporary arrest can be expected.

The worst possible cases are those in which, during some months

or years, one eye becomes gradually blind, with irregular lateral contraction of the field of vision, diminution of S., and atrophy of the disc; and in which the second eye follows the first at an uncertain interval.

There are, finally, many patients who seek relief, not on account of obviously defective sight, but on account of fatigue and pain which attend upon, or even altogether forbid, any prolonged exertion of the eyes. Some of these cases depend upon unsuspected hypermetropia or astigmatism, either of which conditions would be detected by the methods of examination already laid down. Others depend upon hyperesthesia of the retina, and are often relieved by blue spectacles. Others, again, depend upon weakness or insufficiency of the internal recti muscles, which become tired, and unable to maintain the convergence of the eyeballs required for close attention.

When the internal recti are weak, if we direct the patient to look steadily with both eyes at some object not much beyond his near point, and if we then interpose a hand, so as to shut out the object from one of his eyes, the eye thus excluded from the visual act will immediately lose its convergence, and wander outwards, usually beyond the median line. Distress from this affection is most common in myopic persons, who require to maintain habitually a greater degree of convergence than is necessary for others.

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ART. XIII.—*Cases in Surgery.* By EDWARD HAMILTON, F.R.C.S.I., one of the Surgeons to Dr. Steevens' Hospital.

#### EXTIRPATION OF THE THYROID BODY.

THE operation of removal of the thyroid body, for the relief of bronchocele, has found little favour in the eyes of practical surgeons, as even the most superficial examination of its anatomical relations and connexions must impress us with the difficulties and dangers to be encountered in such a proceeding; the close proximity of the organ to the carotid artery, jugular vein, the trachea, larynx, and œsophagus; the large supply of vessels, both arteries and veins, so freely distributed to it from various sources, are almost sufficient in themselves to proscribe any operation for its excision; and when we consider that the diseases to which the organ is liable, although producing most unsightly deformity, do not usually threaten life or



interfere with vital functions for a considerable period of their duration, we cannot wonder that opinions such as the following have been expressed against operative interference:—

“Excision of the tumour is seldom to be thought of.”—Erichsen.

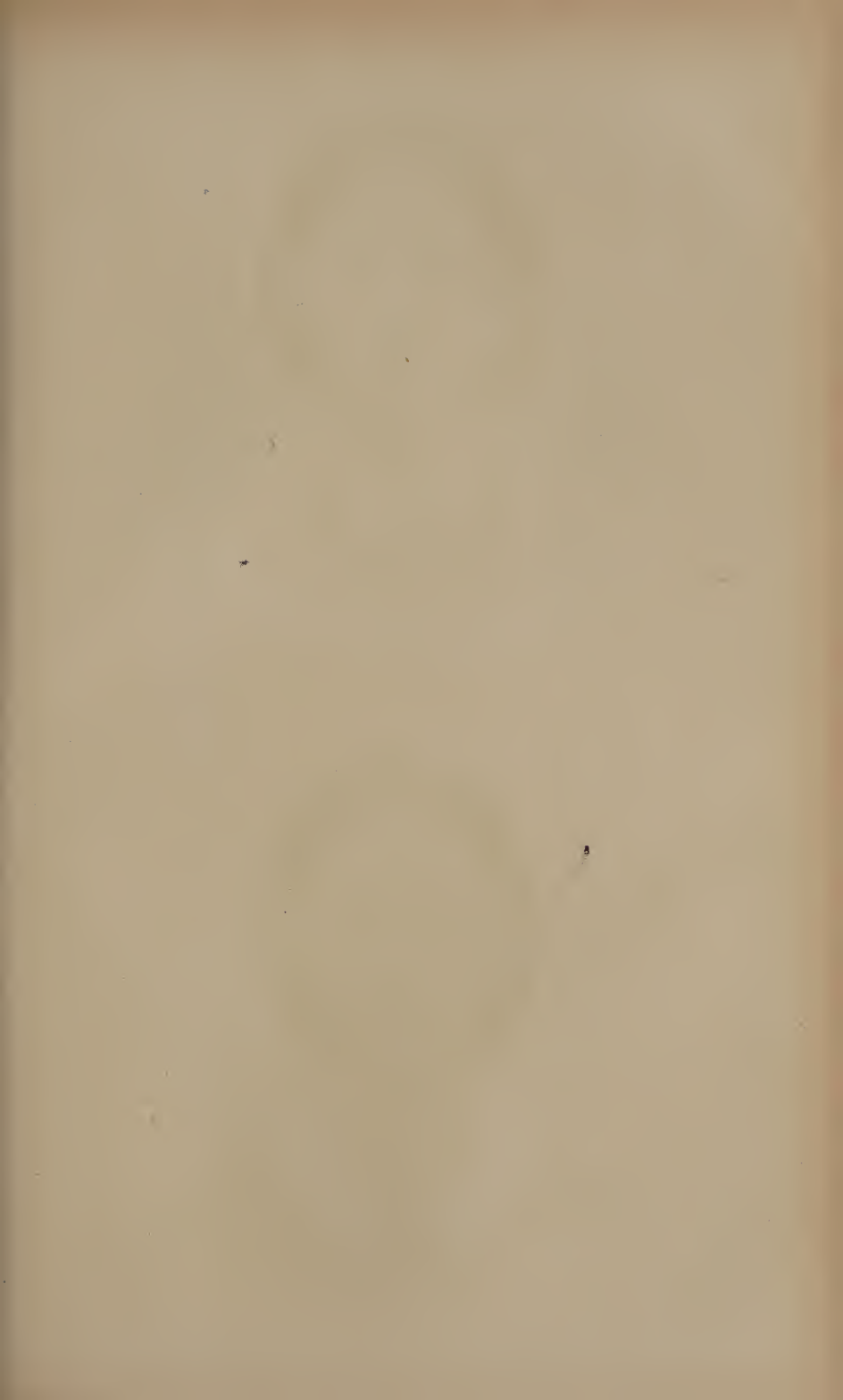
“The disease is such as should not at once be removed with knife, nor indeed should any attempt of the kind ever be made excepting under peculiar and urgent circumstances.”—Fergusson.

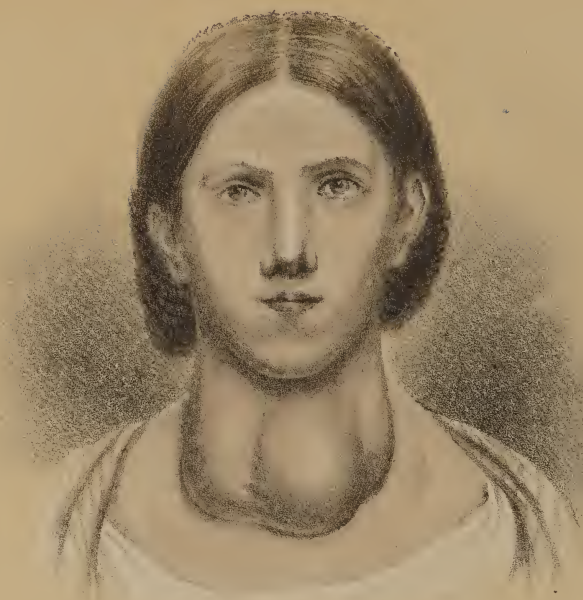
“No operation is justifiable in such cases for the removal of deformity.”—Liston.

“Attempted removal by knife would scarcely fail to prove fatal by hemorrhage.”—Miller.

“Very dangerous and almost entirely to be forbidden.”—Chelius.

Forcible interdictions such as these, emanating from the highest authorities in our profession, must command the deepest respect from every prudent and conscientious surgeon, and make him pause and anxiously deliberate before he determines on an undertaking so fraught with dangers and difficulties, and even cause him to shrink from it altogether; and yet cases occasionally present themselves to our notice which appeal so plaintively and forcibly to our sympathies as to make us reluctant to acknowledge the limited resources of our art, when we pronounce them beyond the pale of surgical interference, and tempt us from the beaten path of routine by a bold and decisive operation to get rid of an intractable disease, entailing on its unhappy victim consequences from which the sensitive female mind recoils almost as from death itself—conspicuous deformity, ever increasing and hopelessly incurable—such thoughts were suggested by the subject of this memoir. The history of the operation, while it records some sad and appalling failures, affords many examples of brilliant success; to none has so much honour in this department of surgery fallen as to Hedenus, who gives the details of six operations, the results of which were satisfactory. Sedillot records three successful cases; Gracfe had two; Voisin, Voget, and Cabaret detail successful results, and many others are recorded in our own periodical literature, which leave us not without hope that in cases selected with care and judgment the operation may be attended with favourable results; and, as stated by Velpeau, we should be wrong in censuring in absolute terms the essays made to render us more familiar with this operation.





M. M., an interesting girl, aged sixteen, resident in the County of Meath, was admitted into Steevens' Hospital on the 10th of May last, presenting manifest enlargement of the thyroid body. The accompanying plate, by Forster, copied from a very graphic drawing by Connolly, will convey a more satisfactory idea of the appearance caused by the deformity than any verbal description. That portion of the enlargement marked A struck me as being more movable than bronchoceles usually are, gliding freely under the skin, as if attached by a pedicle; some large veins coursed over its surface. The tumour marked B seemed to be more fixed, to have much deeper connexions; the movement of this also produced some pain. Both portions of the tumour followed readily the motions of the larynx in deglutition; they were soft and elastic at points, giving an obscure sense of fluctuation. No pulsation or bruit could be observed. She suffered much from headaches and dizziness. The uterine functions were regularly performed. There was no evidence of cardiac disease, nor any proptosis. She stated that the disease was of six years standing; that she had been treated for it in many different ways; the swelling some times diminishing, but quickly regaining its size.

She was much fretted at the prospect of the continued deformity, and expressed her most urgent desire to submit to any operation rather than remain in her present condition. The extreme mobility of the tumour suggested to my mind the idea that it might be eradicated without any dissection after the free division of the integuments, and that the subsequent steps of its removal might be accomplished by the use of the *écraseur* without any serious risk of hemorrhage, which constitutes so formidable an obstacle to the proceeding.

While engaged writing this memoir I have found that I have been anticipated by Dr. Voss, who records a case thus treated with perfect success in *American Medical Times*, 1862. Having explained my views to my colleagues, in consultation, it was decided that the operation might be undertaken. The presence of the catamenia obliged us to defer it for some days.

On the 10th of June, chloroform having been administered, the patient lying on her back, a pillow placed under her neck, with the head well depressed, so as to make the tumour as prominent as possible, an incision three or four inches long was made in the middle line directly down on the tumour; the fascia connecting the sterno-hyoid and thyroid muscle was carefully divided on a



director to the same extent as the cutaneous incision; the finger was passed around the tumour on the left side, which was easily turned out; that on the right side with somewhat more difficulty, yet, still with wonderful facility; as they both protruded through the wound they pulsated very strongly, large vessels entering from all directions. Considerable bleeding issued from the lower part of the incision—bright, but yet not projected per saltum; there was also smart hemorrhage from the upper part; this must have proceeded from a number of small veins torn in turning out the tumour, as all use of the knife was avoided after the fascia was freed; the loss of blood was so great that we did not consider it advisable to wait for the slow action of the *écraseur*, but having raised up the tumour we passed a strong ligature around its neck, and proceeded to tie it firmly. This seemed to interfere in an alarming way with respiration, producing cough and much distress; we at once removed it, and having transfixed the narrow part with a ligature of very thick silk, tied each segment separately, and then cut away the tumour in front of the ligature. The surface bled very freely after its removal, but pressure commanded it without much difficulty; the patient was exceedingly faint from loss of blood; the wound was left exposed to the air, ice applied to the surface, and an intelligent dresser directed to remain constantly at the bed-side, and control any bleeding which might arise by pressure; bloody serum oozed for some time, but no further hemorrhage occurred. The great faintness required the constant administration of brandy, with small doses of opium.

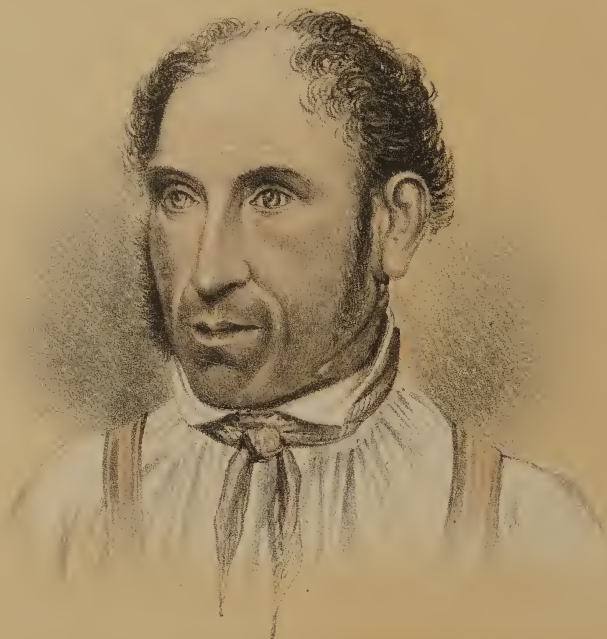
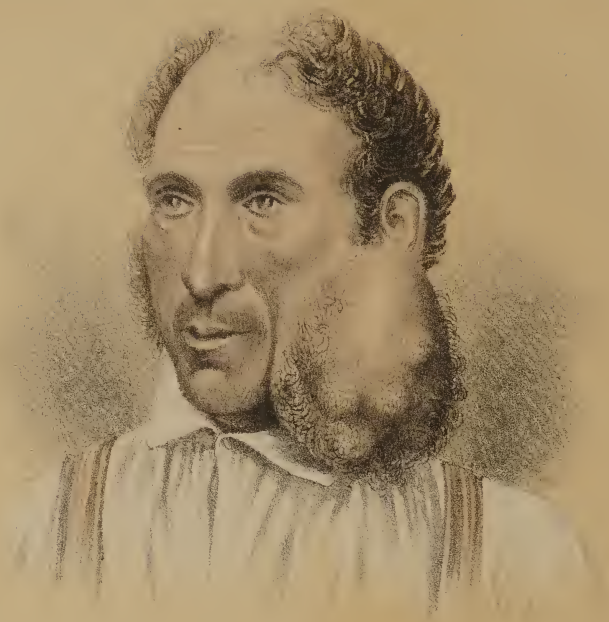
*Evening visit.*—Patient has had some sleep; pulse 130; very thready. There had been some slight hemorrhage from the wound, induced by an attack of vomiting, which was easily arrested by digital pressure. The sides of the wound were supported by a single strap of adhesive plaster, but not brought together; a piece of ice on muslin was laid over it.

Ordered to continue brandy and opium, with strong beef-tea as a drink.

June 11.—Passed a quiet night; no return of bleeding; reddish serum in large quantity flows from the wound; pulse 120. The brandy to be given at longer intervals; to have beef-tea and arrow-root; complains of some soreness in swallowing.

*Evening visit.*—General appearance much improved; pulse 110; complains of some soreness in the wound, the edges of which present a slight blush of inflammation. Omit brandy and opium.





June 12.—Continued improvement; pulse 100, full; complains of cough, which causes much distress, disturbing the wound. Ordered to have inhalation of steam, with a little gum in the mouth.

*Evening visit.*—Cough much relieved; less difficulty in swallowing; pulse 100. To have a full opiate; wound to be poulticed.

June 13.—Passed a very good night; pulse 98; wound suppurating; discharge somewhat fetid; cough very slight. Ordered warm dressing to the wound, with a linseed poultice; to have wine, with arrow-root, beef-tea.

June 14.—Wound suppurating freely, with a grey slough in the centre; pulse 95, firm; was somewhat restless in the night. Ordered an aperient draught, with sulphate of magnesia and infusion of roses.

June 15.—Passed a much better night; quite cheerful; pulse 90; ordered to have chicken.

June 16.—From this date the reports presented little variation of any interest, each day exhibiting marked improvement.

June 21.—Ligatures came away, being the 11th day after the operation. Allowed to leave her bed.

The lithograph shows her appearance on the 6th of July, before she left the hospital. A cross, suspended from a neck ribbon, served completely to conceal the small line of cicatrix which remained. She was much pleased and happy in her mind at the removal of the deformity.

On examining the tumour, the movable portion (A) presented the cystic variety of bronchocele, the other exhibited simply hypertrophied thyroid structure.

#### A LARGE TUMOUR OF THE PAROTID REGION SUCCESSFULLY REMOVED.

J. N., aged fifty, a strong healthy-looking man, residing in the country, presented a large tumour, occupying the lower part of the parotid region on the left side; it was globular in shape, and somewhat nodulated—the projections conveying a feeling of hardness. The integument covering it was vascular, but did not adhere to the tumour at any point. It was not tender to the touch, nor did it occasion any pain or inconvenience by its pressure, the deformity alone urging the patient to seek relief.

He states that it commenced, without any assignable cause, twenty or twenty-five years ago, as a small pea-like body, movable



under the skin; that its increase for the last two or three years has been very much more rapid than previously; his general health has always been good; had fever before the disease appeared. On careful examination the tumour was found to be perfectly movable and unconnected with any important structures, the external jugular vein was the only vessel of any size in danger; its removal was accordingly at once determined on. On Saturday, August 5, chloroform having been administered, a curved incision was made obliquely over the tumour, from the lobe of the ear to an inch in front of and below the angle of the lower jaw; a second curved incision between the same points included an oval portion of the skin, which was removed along with the tumour; the skin was carefully dissected back from each side, while the growth was steadied with a vulsellum, and well raised from its bed by my colleague, Dr. Symes; by means of the finger passed around it, and a few touches of the knife, it was turned out without difficulty. The bleeding vessels were now carefully secured, requiring five ligatures. Cold water was poured over the bleeding surface to wash away clots and cause immediate contraction of the smaller vessels, as I was most anxious to avoid any collection of blood in the cavity which might tend to unhealthy inflammation. The external jugular vein appeared of large size at the bottom of the wound, but was carefully avoided; the edges were united by sutures of silk, a large compress being placed over each flap, and finally secured in its place by a bandage, so as to prevent pouching. The patient was placed in bed, and a full opiate administered.

*Evening visit.*—Has been very easy, having had some sleep; pulse 90; no appearance of bleeding.

August 7.—Passed a quiet night, is now somewhat feverish; skin hot; tongue coated; pulse 100. Stitches were all removed—a process much facilitated by having left one end of the silk sufficiently long for traction. Cold was still applied to the surface.

8th.—Skin cooler; tongue cleaning; no pain in the wound, which was ordered to be poulticed every six hours; to have beef-tea.

9th.—Wound suppurating freely, matter discharged along the ligatures; treatment continued.

10th.—One ligature away.

14th.—Two ligatures away.

16th.—Remaining ligatures detached.

The tumour weighed 1lb. 4oz.; its external surface was vascular; on section it presented a whitish granular appearance, resembling

boiled rice; a soft pulpy material oozed from it; the granular matter was composed of small cyst-like bodies; the denser nodulated portions cut like true cartilage, and exhibited, under the microscope, cells almost identical with that structure.

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ART. XIV.—*Diphtheria and Diphtherial Paralysis.* By W. J. CUMMINS, M.D., Member of the King and Queen's College of Physicians; Physician to the Cork Dispensary, &c., &c.

THE question is often asked, "is not diphtheria a new disease?" and in Ireland it may be answered that it is still so far new that many physicians of experience have never seen a case, while to the majority the instances that do present themselves appear rather as curiosities than as harbingers of a pestilence which seems gradually establishing itself amongst us. But although unknown until lately by the present generation, diphtheria is by no means a new disease, for besides having been described by Fothergill in 1728, and in France, by Bretonneau, under the name of "Diphtherite," in 1821, accounts of various epidemics are to be met scattered through the medical literature of Europe for the last 350 years—(Gibb<sup>a</sup>). Bretonneau, in his second memoir, tells us that "ever since the end of the sixteenth century diphtherite has almost constantly shown itself in every region of the old and new continents. At first it continued for a long time in Spain, and during nearly forty years was noticed in different parts of the Peninsula. Rather later all Italy was successively afflicted by it. . . . In two years it killed more than 5,000 persons in Naples. . . . Towards the middle of the last century epidemics prevailed in England, France, Sweden, and America."

Since the re-appearance of this disease it seems to have spread in England and Scotland much more rapidly than in Ireland—where, judging from the Registrar-General's report for 1864, it has prevailed more extensively, and been more fatal in the north than the south.

We find, however, occasional mention of it in the Transactions of the Cork Medical Society, which, as a permanent record of the

<sup>a</sup> Diseases of the Throat and Windpipe.

diseases of the South of Ireland, has proved so valuable. Professor O'Connor read a paper on the subject in 1860, describing a localized epidemic which occurred in a boarding school near Cork, due to an importation of the disease from Dublin.

Dr. William Townsend described an epidemic which prevailed in the Cork Union in December, 1862, and laid before the society specimens taken from two fatal cases. He remarked that "in some cases the patients are quite amenable to treatment, and yet sink rapidly; in others they are restless and unmanageable, the disease extending into the larynx and air tubes, the breathing becomes difficult, and the patient dies asphyxiated. . . . In some cases treatment seems to be of no avail, the patients sinking without an effort, as if the local symptoms were merely the outward manifestations of a poisoning of the system. In others, after a struggle of several days, the severe symptoms subside, &c., &c. . . . In my experience," he adds, "the convalescence is greatly protracted." Professor O'Connor says, that in all his cases the tonsils were coated with lymph, and that the pharynx was "more or less inflamed, and of a dark red colour." He dwells much on the "predominance of the nerve-poisoning over the local symptoms," all the cases presenting a peculiar expression of languor. In the same Transactions we find an account of an epidemic which occurred in the practice of Dr. Luther, of Clashmore, Co. Waterford, in 1864, paralysis occurring as a sequel in some cases.

Dr. N. Hobart mentioned, last session, at the society, that he also had seen a number of cases about the same time in the Douglas Dispensary district. On the whole, very few have occurred among the better classes in Cork, and such as have, generally proved fatal, so as to afford no time for a study of their sequelæ.

During last Winter diphtheria broke out in Blackrock, about a mile or so from Cork, and several cases occurred rapidly, one after another; while, at the same time, sore throats of a most severe description, many of them presenting obscure traces of a diphtheritic character, were prevalent.<sup>b</sup>

The first case of true diphtheria, a child about two years of age, was remarkably free from any pellicular exudation on the throat,

<sup>a</sup> Dr. Greenhow, in his work on diphtheria tells us that Dr. Fleming, when Professor of Materia Medica in Cork, saw two well marked cases of diphtheria in the year 1856.

<sup>b</sup> Dr. Jenner notices the prevalence of sore throats during epidemics of diphtheria, and suggests a resemblance in this particular to the diarrhea which accompanies epidemic cholera.

which although very much congested could not be recognized as due to a specific disease. There was much attendant debility; and after about a fortnight, when the local soreness had gone, an almost complete inability to swallow set in, fluids returning, on the instant, through the nose; at the same time there was great weakness of the lower limbs, with general feebleness and anemia. A day or two afterwards there was complete paraplegia, involving both sensation and motion, the anesthesia extending upwards almost to the umbilicus. Next morning I was sent for in a great hurry, dyspnea having set in the night before, and increased rapidly. The patient was livid and gasping when I saw him; quantities of frothy mucus so completely filling the mouth that it was impossible to view the throat. In less than half an hour he died.

About the same time I attended a young lady who had the membranous exudation of diphtheria upon the back of the pharynx, but recovered rapidly. Her mother, who slept in the bed with her, was next attacked, and suffered much more severely, the exudation being like moist chamois leather on the pharynx, and here and there on the tonsils and palate; while the gums, hard palate, and inside of cheeks were covered with a thin, pearly-coloured membrane, which seemed almost transparent in some parts.

The local affection lasted for nearly ten days in this case, when it gradually subsided; a small abscess containing healthy pus forming on the anterior surface of soft palate. During the greater part of her illness this lady was prevented from obtaining any rest by the quantity of "scalding" fluid which exuded from the throat and mouth, obliging her to keep her head continually over a vessel. The servant who attended her was next affected; the disease assuming the laryngeal form, marked by croupy breathing and cough. She also recovered. At the same time a lady from the country brought her little girl to spend a week with some friends residing on the same terrace with the family just mentioned. They returned home on Saturday, on which morning the little girl, before starting, complained of her throat. Towards evening she became feverish, and spent a restless, delirious night, complaining much of her throat. Next morning the family physician saw her. On Monday she was so much worse that another medical man was called in, but without avail, as she died asphyxiated about twenty-four hours after. The local disease could not have presented an appearance very characteristic of diphtheria, as her illness was supposed to be suppressed scarlatina. That, however, is no proof that



it may not have been a case of diphtheria, as it is well known that the peculiar throat affection may be entirely absent in that disease.<sup>a</sup>

This child was carefully watched and attended by her mother, who breathed her breath continually, and thus absorbed the poison which had proved so fatal. She was next attacked, and then the whole family commenced taking the bi-sulphite of soda, and continued it steadily for many weeks. They all escaped. On the third day of this lady's illness (January 21st) I was telegraphed for, and met the family physician in consultation that night. The patient was in an exceedingly low, nervous condition, complaining much of tightness about the upper part of the chest; breathing laryngeal, and much oppressed; thin diphtheritic exudation appeared on the alveoli; and the throat was partially covered with a more dense membrane, which hung loose at the left side, where it was also most completely formed; the parts of the throat free from exudation were of a dusky red colour. I removed the loose pellicle at the left side, with forceps, and prescribed a gargle of the muriated tincture of iron, which brought away much more of the same. I also suggested, for the consideration of the medical attendants, at their meeting next morning, the internal use of the same tincture. I was subsequently informed that she was much worse the following day, the breathing being more oppressed; the gentlemen who had charge of the case then prescribed the internal treatment I had recommended, when she began to improve, and progressed steadily and favourably towards recovery; so much so that in three or four weeks she was able to swallow perfectly well, and drive out every day. Towards the end of February I was written to about a fidgetty nervous sensation in the lower limbs, which was relieved immediately by friction with an anodyne liniment. Shortly after the catamenia appeared, and from that time forward there was a gradually increasing difficulty of deglutition. Solids, such as meat, could not be swallowed at all, and fluids of all kinds returned

<sup>a</sup> The membrane of diphtheria is met on the skin, interior of ear, vagina, rectum, &c., without any throat affection; but even when the latter is the part attacked there may be nothing to distinguish it from an ordinary inflammation.

Such a case is given by Dr. Latham Ormerod, in the *Lancet* (December 7, 1861). The patient was in hospital throughout his whole course, and was supposed, for some days, judging merely from the appearance of his throat, to have common quinsy; yet he had subsequent paraplegia and paralysis of the soft palate, just as after typical diphtheria.

immediately through the nose.<sup>a</sup> I prescribed (by letter) in the commencement of these symptoms, pills containing quinine and ext. nucis vomicæ (half a grain of the latter) three times daily; but the disease still continuing to progress, I ordered her up to town, to be near me.

She arrived on the 3rd of March in an exceedingly weak, anemic condition; pulse feeble, 120; breathing rapid and short; voice exactly that of a person with cleft palate; unable to swallow solids, while fluids returned in streams through the nose, not a drop passing down. Semi-solids can be swallowed in teaspoonfuls. There has been occasional dimness of vision, and a sensation of numbness in the upper part of right thigh, felt only for a short time after she wakes in the morning. The symptoms are decidedly on the increase; there is great depression of spirits, and a tendency to constipation. Some hardness and fulness of the cellular tissue, beneath the jaw, exists, and prevents her opening the mouth fully. This was ordered to be rubbed with a liniment composed of half a drachm of the linimentum iodi of the British Pharmacopeia; and seven drachms and a-half of camphor liniment, and a pill to be taken thrice daily, composed of one grain and a-half of the extract of nux vomica of the British Pharmacopeia, two grains of sulphate of quinine and extract of hyosciamus.

Beaten up eggs, jelly, beef-tea thickened, port wine, and semi-solids of various kinds were taken in teaspoonfuls continually.

Next day, March 4th, I increased the dose of nux vomica to three grains.

On the 5th she was much the same way; but before I left the house had an attack which seemed, at the time, to be hysterical, but which, when it afterwards recurred, I found was due to the pills.

Dr. E. Townsend saw her with me that afternoon, and coincided in my opinion as to the nature of the case and its treatment. He continued in attendance, and took much interest in watching its curious phenomena.

7th.—Each pill has since produced the nervous symptoms, simulating hysteria, as well as twitching of the voluntary muscles.

8th.—The effect of the medicine increases.

10th.—The physiological effect of the nux vomica which she has

<sup>a</sup> "The medulla oblongata appears to be the centre whence are derived the motor impulses, enabling the muscles of the palate, pharynx, and cesophagus to produce the successive co-ordinate and adapted movements necessary to the act of deglutition."—Kirkes' Manual of Physiology.

since been taking in three grain doses, three times a-day, is most marked and curious. It commences by day, from an hour to an hour and half after taking the pill, and continues from an hour to an hour and half. The pill taken at bed time does not begin to affect her till two or two hours and a-half after taking it, when it wakens her out of sleep, and keeps her awake a little more than an hour. During the intervals of the action of the medicine she is *quite* free from its effects, but while under its influence her condition is most remarkable. She calls it "her electricity," a name most appropriate. If she attempts to convey a spoonful of food to her mouth it is carried with a jerk to her nose, or some other part of the face; if she attempts to walk her limbs start away from under her; sometimes she jumps up with a start and a scream, caused by sudden spasm; hyperesthesia of the cutaneous surface, especially of the face, exists to such an extent that if touched "a thrill is sent to every part of the body." Throughout the primary illness the left side was more affected than the right, and it is so still; but she describes the spasms as commencing sometimes in the eyes, and sometimes in the back of the neck. The swelling below the jaw has almost disappeared, and the power of swallowing is gradually returning; the tone of voice has changed from that of a person with cleft palate to that of one with too large a tongue. She also complains of the left side of tongue feeling numb and enlarged; but on examination it is not found to be so.<sup>a</sup>

13th.—Since last report the pills have been taken regularly three times a-day, and an hour or so after each the phenomena above described have regularly occurred without any increase in duration or intensity until this day, when the afternoon pill acted much more vigorously than any former one, its effect also continuing several hours. There is now a most decided improvement in the power of deglutition, a large part of the fluid taken passing down; pulse is reduced to from 80 to 90, and the tongue has cleaned. The respiratory distress has almost quite gone.

14th.—She was desired to take only one pill in the afternoon to-day, the effect of which was not more severe nor permanent than on the days preceding yesterday.

<sup>a</sup> This sensation was caused by an affection of the hypoglossal nerves, derived from the medulla oblongata. "Schroder Van der Kolk has brought forward much evidence to prove that the corpora olivaria are accessory ganglia, the main use of which is to communicate simultaneously to the hypoglossal nerves the multitudinous combinations of muscular movements required in speech, and thus produce a uniform action of both sides of the tongue."—Kirkes' Manual of Physiology.

15th.—To take one pill twice a-day.

18th.—Pills have been continued twice a-day since, each one producing the same effect as before the 13th; the catamenia being now due they were omitted entirely.

20th.—Catamenia present. She can now drink perfectly well, but complains a great deal of numbness of feet and hands. She also, occasionally, sees double when looking at an object five or six yards distant, although she can see well enough to read and sew. She cannot walk as well as before, and is unable to feel the ground under her; so much so that she would fall if she did not regulate her movements by her eyes. The feet also feel heavy. To return to the pills twice a-day.

22nd.—Pills affect her just as before, only with greater intensity and duration; so much so that the effect of one dose almost overtakes that of the next. I accordingly omitted them and prescribed one of half the strength twice daily.

25th.—The pills containing one and a-half grains *nux vomica* have produced no effect; numbness of limbs have increased, and she seems anemic and weak. I prescribed twenty drops of the tincture of the perchloride of iron three times a-day, and to continue the half pills twice a-day.

April 2nd.—The half pills having produced no effect I desired her to return to the three grain pills twice a-day.

3rd.—Has taken only two pills, each of which produced most violent effects; so I directed only half a pill to be taken three times a-day.

13th.—Much better; general health improving greatly; voice and deglutition have continued perfect; amblyopia is never complained of now; sensibility of feet and hands is returning fast, and power of walking is increased. Has been taking the one and a-half grains of *nux vomica* three times a-day ever since, with no increase of physiological effect, and with decided benefit in every way.

I made no further note of the case, but my patient steadily improved, and with the exception of a sensation of weight in the feet, on over exercise, for which she takes hot salt-water baths, health is now (June 27th) perfectly restored.

This case confirms an observation of Dr. Jenner, that when one side of the throat is more severely affected than the other by the primary disease the subsequent paralysis is most severe at that side; but the most interesting point in its history is the peculiar action



of *nux vomica*, which struck both Dr. Townsend and myself as being something unusual.<sup>a</sup>

It may be well briefly to review the case with special reference to this point, as it is extremely interesting in its bearing upon therapeutics and toxicology. The patient had been taking half a grain of the extract of *nux vomica* of the British Pharmacopeia while in the country, thrice a-day; after her arrival in town she took, during the first day, a grain and half, and during the second three grains three times a-day, after which the physiological effect of the medicine was first noticed. This, as has already been described, commenced from an hour to an hour and half after each dose by day, and lasted about the same time; at night the interval between the dose of the medicine and its effects was rather longer. When three grains, three times a-day, had been taken, during nine days, the effects had arrived at their maximum, and one dose had begun to overtake another in the system. The effects attributed to each pill could not have proceeded from any other cause, as the regular sequence of events occurred too frequently not to establish a connexion between them; but if there was room for doubt the after history of the case completely removed it. Thus—the pills were omitted for two days during menstruation and there were no symptoms such as had invariably followed each dose; they were then resumed *twice* a-day, and each produced a most intense and unmistakable effect. The dose was then reduced to a grain and half *twice* a-day, and continued for eleven days without once manifesting the least effect; the patient then returned to the pill of three grains twice a-day; two doses only were taken when they had to be discontinued, so severe had been the effect of each. A grain and half was then taken three times a-day, and after ten days it was noted that the physiological effect of the medicine had not increased, that the general health had greatly improved, and the paralysis almost disappeared. The preparation used in this case was that of the British Pharmacopeia, and was procured from Mr. Brady, of Bridge-street. If we could always judge as accurately, from the immediate effect of poisonous medicines, when to increase and when to diminish the dose, they would be more manageable than we generally find them. Judging from this case it would seem that the

<sup>a</sup> From the evidence of the celebrated Palmer trial it would appear that from half to three quarters of a grain of strychnine is sufficient to kill a human being, and that from five minutes to an hour may elapse, according to the quantity administered, between taking the medicine and its first effect.

extract is much to be preferred to the alkaloid strychnia in diphtherial paralysis, as it is more manageable. The gradual cumulative tendency of the medicine is also well illustrated, as towards the end of the treatment the dose which had before produced simply physiological effects acted so powerfully as almost to amount to poisoning; and that a portion of the medicine was retained in the system after it had been discontinued for two days, is proved by the complete intolerance of three-grain doses when resumed after that interval.

Paralysis such as we have described, during convalescence from diphtheria, commencing with the muscles of the soft palate and constrictors of the pharynx, and gradually extending, is not uncommon. A similar one has been graphically described in the *Edinburgh Journal*, Vol. CXVII., by Dr. Joseph Bell. Another of the same kind, but less severe, is so well related by the sufferer himself in the following letter, that I transcribe it. He says:—

“I had a very bad attack of diphtheria, and lost twenty-four hours of skilful treatment. . . . C. at once used caustic most severely, and continued so to use it for several days, and thus checked the disease. I suffered a great deal from the pain of swallowing, but did not otherwise feel very ill at the time; but after my throat began somewhat to recover, and after I had been able to return home, I began to experience an extraordinary difficulty of swallowing. Soon after I began to feel what I thought was merely weakness, but which was, in fact, incipient paralysis in my legs, and I had great difficulty in getting up and down stairs, and into my carriage (a waggonette with one step). Gradually this extended to my hands, and I could with difficulty button my clothes. I could scarcely write, and then an almost illegible scrawl; and all these symptoms kept on increasing while the local soreness of the throat was quite healed. I wrote for advice to C. He ordered me change of air and scene, to take a bath morning and evening as hot as I could bear it, with a person to rub me hard while in the bath. I got myself regularly rubbed by a strong coachman, who *curried* me pretty much as he would a horse; this in the bath, with water as hot as I could bear it. I took as much nourishment as possible in small quantities frequently (especially flummery with cream, morning and night). I moved about, first to A., then to L., then to D., intending afterwards to try the Turkish bath at Blarney, but I got so much better in the

course of a month of touring about, always keeping up the treatment as to hot bathing, and frequent feeding, also taking syrup of bark and some other tonics, that I found I had no occasion to resort to the Turkish bath."

Dr. O'Connor describes the paralysis which occurred in one of his cases as follows:—

"In about ten days from the commencement of the attack, this child had apparently recovered. Her throat was quite well—there was no difficulty in swallowing—still the voice had not its natural tone, and there was great muscular debility in the whole body. When walking she occasionally tottered in her gait, and felt a reeling in her head, which made her unwilling to leave her chair. She complained of motes flying before her eyes, and sometimes lost her sight for several minutes, accompanied with a ringing in her ears of a most distressing kind. In this case I examined the state of the urine, which contained no albumen. The child continued in this most distressing state nearly three months, although during the entire time her appetite was very good, and her general appearance showed no sign of delicacy. She finally recovered perfectly."

Dr. Kidd, of Dublin, in a letter, which he has kindly permitted me to make use of, says:—

"I had a very interesting case of paralysis after diphtheria some months ago in a lady aged about forty-five or fifty. The primary attack was slight, and occurred in October: my attendance began on the 6th and ended on the 15th. In the middle of December she began to complain of numbness in her feet, and cramps in her legs, gradually extending towards the knees, and soon affecting the hands; this was accompanied with great muscular weakness; she drove to my house on 21st December, having walked part of the way from R., but in the following week she could not go up or down stairs, and the want of power increased so much that she became unable to walk across the floor without help; several times she fell, and had to be lifted into bed, but could move her limbs freely while lying there; sensation seemed perfect; when tried with a pair of compasses, she could recognize the distance of the points all through her illness on both hands and feet. The power of directing movements was also perfect; she could lift a pin and hold

it; and could touch any part of the floor she aimed at with her toe; at no time was there difficulty in swallowing; once the throat healed; pulse ranged from 120 to 100—small and feeble. I treated her with citrate of iron and quinine, as recommended, I think, by Jenner. She herself dated the beginning of her improvement from the action of a dose of blue pill and colocynth. I also made her bathe the feet and legs in mustard and water every night, and use a stimulating liniment. She made a perfect recovery. It seemed to me to be muscular debility from blood-poisoning rather than nervous."

This last remark of Dr. Kidd's suggests for our consideration the very important question:—What is the cause of diphtherial paralysis? We have now had the particulars of several cases before us, and many more may be studied in the works of Dr. Greenhow and other writers on diphtheria; but before entering into the question, it may be well to quote from the year book for 1863, a sort of summary taken from a paper by M. Roger (*Archiv. Gen. January and February, 1862*), who gives thirty-six cases, viz.:—twenty-seven of the throat and soft palate, sixteen of which died; and seven of general paralysis, of which two were fatal. The same author tells us that the localization of the primary diphtheria was pharyngeal in twelve cases; laryngeal in twenty-three; and cutaneous in two. That among the twelve pharyngeal, paralysis of the velum palati followed ten times; palsy of the sphincter ani once; and amblyopia once. That four of the ten cases of velum paralysis were uncomplicated; four had extensive paralysis; and two, paraplegia. In the pharyngo-laryngeal cases there was almost always paralysis of the pharynx. One child who had exudation of the mastoid region, and external auditory meatus, suffered from palatal paralysis and throat affection. The rectum and bladder were paralysed in two instances.

The various cases of diphtherial paralysis now before us afford ample opportunity of studying the clinical features of the disease, which we find to correspond more with those forms of paralysis denominated functional, than with those which depend upon organic disease. Dr. Handfield Jones tells us, in his excellent work on the nervous system, that in the published *post mortem* examination of cases of diphtherial paralysis no organic change has been found in the nervous centres, except in one case where hemiplegia of the right side was found associated with a small suppurated spot in the



left cerebral hemisphere. This was probably a coincidence; so we may conclude that diphtherial paralysis is functional, and proceed with our investigation as to its cause.

Virchow (*Arch.* 25, p. 114, 1862),<sup>a</sup> suggests that it may depend on primary peripheral alteration of the nerves, propagated from the originally affected parts to the special centre, much in the same way as in tetanus the irritation is propagated from the wound.

The experiments of Comhaire, more than half a century ago, and those of Brown-Séquard, of the present day, have demonstrated the possibility of paralysis being caused by irritation, propagated along a sentient or afferent nerve to a nervous centre, the effect being manifested in the parts supplied by that centre. The latter great nervous pathologist has explained this apparent anomaly by proving that the immediate effect of the irritant in such cases is a spasmodic contraction of the blood vessels supplying the centre, and loss of its function consequent on imperfect nutrition. Such an explanation might be applied to diphtherial paralysis, if the latter occurred during the existence of the primary local disease; but it has been shown that it does not generally occur until all local irritation has subsided, and it is known that as long a period as two months (Jenner), or even four months (Eade), may elapse between the primary disease and the consecutive paralysis.

Dr. Gull suggests that the paralysis of diphtheria may be due to the local affection extending by continuity of structure from the fauces to the upper part of the spinal cord; and it has been also supposed that the original zymotic poison may directly affect the nervous centres, and cause paralysis in the same way that other poisons, such as urea, lead, or arsenic produce it: but the length of time which elapses between the primary and secondary affections negatives both these hypotheses. Dr. H. Jones (*ante cit.*) thinks that some special modification of the original poison may be generated in the system, and the occasional occurrence of albuminuria at all periods of the disease, has, with some show of reason, been connected with the palsy. But paralysis often occurs after diphtheria where there has been no albuminuria (Sanderson, Roger), and the symptoms which accompany the paralysis do not afford any confirmation of Dr. Jones' supposition, while the frequency of recovery, without the administration of any chemical antidote, or any special evacuation from the system, is an additional reason for

<sup>a</sup> Year Book, 1863.

believing that no poison has been generated in the blood. To understand the sequelæ of diphtheria we must go backwards and study the nature of the original disease, or rather of the class of disease to which it belongs.

In zymotic diseases we have manifest evidence of a poison received from without, contaminating the blood—reproducing itself a hundred fold upon some constituent of that fluid; and, in doing so, destroying that constituent for a time. That this series of events takes place is abundantly proved by the result of inoculation with various poisons.

Thus—a successful vaccination leaves the system to all appearance in the same state that it was before, but in reality minus a something, that something being the constituent of the blood, at the expense of which the virus had been reproduced.<sup>a</sup> The same operation cannot be again performed successfully for many years because this constituent is wanting.

Now if we suppose the “something” which is absent from the blood after vaccination to be a constituent essential to the nutrition of any particular part of the body, it is obvious that the function of that part will be in abeyance until the element of its nutrition is restored?

Let us apply this to diphtherial paralysis: the zymotic poison of the disease undoubtedly reproduces itself within the system; to do so, it must diminish or destroy some constituent of the blood. Subsequent events occur which may be explained by a want of due nutrition of the nervous system. May we not then fairly infer that what has been taken out of the blood is what the nervous system requires? Now it is quite evident that the poison of diphtheria does not entirely destroy that constituent of the blood on which it

<sup>a</sup> “The assimilation of the new materials to the blood must be perfect in regard to all those immeasurably minute particulars by which the blood is adapted for the nutrition of every tissue, and the maintenance of every peculiarity of each. How precise the assimilation must be for such an adaptation may be conceived from some of the cases in which the blood is altered by disease, and by assimilation is maintained in its altered state. For example—by the insertion of vaccine matter the blood is for a short time manifestly diseased; however minute the portion of virus, it affects and alters in some way the whole of the blood. And the alteration thus produced, inconceivably slight as it must be, is long maintained, for even very long after a successful vaccination a second insertion of the virus may have no effect, the blood being no longer amenable to its influence; because the new blood, formed after the vaccination, is made like the blood altered by the vaccine virus; in other words, the blood exactly assimilates to its altered self the materials derived from the lymph and chyle.”—Kirkes’ Manual of Physiology.

acts; it is not a disease like small-pox or scarlatina, which rarely occur more than once in a lifetime, and when they do, in a modified form; on the contrary, relapses are not infrequent, and a second attack is often as severe as the first. We may conclude, therefore, that the material on which the poison of the disease reproduces itself is regenerated in the blood, within a short period—probably a few months—and, that although always diminished, it is rarely, if ever, completely destroyed. This explains why paralysis does not always follow diphtheria, and also why some nerves suffer more than others.

The saying of Treviranus, that each part of the body, by taking from the blood the material it requires for its maintenance, stands in the relation of an excretory organ to the rest, may be applied in altered sense to the subject before us; for when the nutriment of any extended tissue, such as the nervous system, exists in the blood in diminished quantity, any part of that tissue which draws from the blood the normal amount of nutriment, does so at the expense of the other parts. Now we may suppose that the nerves of the throat suffer, in common with all its tissues, in the local affection of diphtheria, and we may well conceive, that in their subsequent debilitated condition, they would be less able to appropriate from the blood the material of their renovation, when it exists in diminished quantity, than parts which had not so suffered.

This may explain what has been noticed in the majority of cases of diphtherial paralysis, that the nerves of the palate and pharynx are those first affected; and it may also help us to understand the phenomena which occurred in the sixth case alluded to in this paper—remarked, too, by Dr. Gibb in one of his cases—that general paralysis may supervene upon a rapid disappearance of the affection of the pharyngeal nerves.

The view we have taken of the pathology of this affection is also borne out by what was more than once noticed in the same case, viz., an aggravation of the paralysis after each catamenial period—the normal loss of blood of course diminishing still further the vitality of the nerves, and thus aggravating the paralysis. Had the latter been due to blood poisoning the menstrual flow must have tended to relieve it by carrying off some of the poison.

This view is further confirmed by the result of treatment, as most cases recover under the influence of nourishment, time, and nerve tonics, not requiring any eliminating remedies. The contrast, in this particular, between the treatment of the sequelæ of diphtheria

and of scarlatina is very great, as we know that in the latter there is no safety for the patient as long as the poison (urea) is circulating in the blood.

It is probable that in many cases of diphtherial paralysis a cure may be effected by time and nourishment alone; but when there is any affection whatever of respiration, it is necessary to stimulate the affected nerves by nerve tonics, such as *nux vomica*, and perhaps electricity. The danger of sudden death is imminent in such cases, and it is fortunate we have a remedy so powerful in its action and so immediate in its effects as *nux vomica*.

But we must not pass over the remarkable tendency to sudden death, which exists both in diphtheria and its sequelæ, with this brief notice, as it is one of the most interesting points connected with the disease. A case has already been given where more than usually complete paraplegia, associated with paralysis of the pharyngeal branches of the pneumogastric nerves, became suddenly complicated by extreme dyspnea, and terminated fatally in a few hours;<sup>a</sup> another has been mentioned where also the respiratory nerves were implicated to a considerable extent, but in the following case it is doubtful whether the sudden death was attributable to paralysis of the pulmonary or the cardiac branches of the par vagum.

Master K., aged two and a-half years, was attacked on the 19th of the present month (June), with what was supposed to be a heavy cold, accompanying dentition. I saw him on the 20th, when a thin ichor was flowing from the nostrils, which seemed to be stopped, causing a peculiar snoring respiration; deglutition was easy, and unaccompanied by pain. On examination, I found the throat much swollen, and of a deep red colour, but could not see beyond the tonsils. The child was anemic and weak, requiring constant support from the first. Next day I was able to examine the pharynx, and found it covered with a thick yellow exudation; this was brushed twice a day with a strong solution of nitrate of silver, while bark and muriatic acid were taken internally. There was also

<sup>a</sup> "Division of both pneumogastric trunks, or of both their recurrent branches, is often very quickly fatal in young animals; but in old animals the division of the recurrent nerve is not generally fatal, and that of both the pneumogastric trunks is not always fatal (J. Reid, *l. c.*); and when it is so death ensues slowly. This difference is probably because *the yielding of the cartilages of the larynx in young animals permits the glottis to be closed by the atmospheric pressure in inspiration*, and they are thus quickly suffocated unless tracheotomy be performed" (Legallois cxxxix.).—Kirkes' Manual of Physiology. May not the death of this young child be so explained?



some tumefaction under the angle of the jaw at both sides. On the 23rd the membrane became loose, and a large thick mass (which the nurse thought was a piece of meat) came away while I was brushing the throat. After this the symptoms were gradually relieved; and although there was partial reproduction of the membrane, it gave no further trouble. The tincture of the perchloride of iron was substituted for the bark mixture, and continued for a day or two; and on the 25th the throat was quite free from membrane, appearing merely congested; in other respects also the child appeared to be recovering fast, although still very languid, weak, and anemic. On that day, however, the nurse called my attention to a peculiar stop in the respiration, which she had noticed occasionally during the last twelve hours. It was simply a cessation of breathing for some seconds, *unattended by distress of any kind, and not followed by gasping, sighing, or any effort to compensate for the time lost.* It appeared more an absence of the necessity for respiration than a difficulty of performing the function.

On the morning of the 26th some spots of purpura were visible on the extremities, but the breathing was improved; throat almost well, and the child seemed better in every respect.

As the pause in respiration, though less frequent, still continued at times, my anxiety as to the result was not removed.

The little patient spent a good day, swallowing quite well, and seeming more lively and like himself than he had been; but late in the evening, while playing on his nurse's lap, he suddenly expired.

It is of the greatest importance to be able to detect any peculiar tendency to sudden death, in time to warn the patient's friends that it may occur, and also with a view of endeavouring to obviate it if possible. The peculiar stop in the breathing noted in this case, may be looked upon as an important element in the diagnosis, and should lead to a peculiarly guarded prognosis.

Its cause may have been either in the cardiac or pulmonary branches of the par vagum. If the former, it was directly due to absence of that stimulus to respiration which the presence in the lung of the blood derived from the right ventricle of the heart affords; and if the latter, to a want of the "impulses producing the respiratory movements," which originate in the medulla oblongata.<sup>a</sup>

<sup>a</sup> "The conclusion therefore may safely be, that this part of the medulla oblongata (the interior of that part from which the pneumogastric nerves arise) is the nervous centre wherein *the impulses producing the respiratory movements chiefly originate*, and whence they issue in rhythm and adaptation."—Kirkes' Manual of Physiology

Paralysis of the cardiac branches of the par vagum is generally indicated for some time before death by the pulse gradually becoming slower and more feeble. Dr. Gibb gives a case where the pulse came down to 32, 24, and even 16, in the minute—the patient at last dying suddenly. Dr. Greenhow mentions a somewhat similar case which terminated suddenly on the 19th day. There had been irritable stomach, vertigo, swimming of the head, and temporary loss of consciousness; on the 17th day the pulse was only 40; on the 18th, 32; and on the morning of the last day of life, as low as 24; suddenly rising to 70 or 80 on the patient being disturbed.

A patient of Professor O'Connor's, when ill about three weeks, had "extreme weakness of the pulse, coldness of the skin, and languor of countenance—not easily explained by the local affection; her intellect was at all times clear, and she talked freely with her attendants, though her utterance was not distinct, owing to the paralyzed condition of the muscles of the palate. While in this state she sat up in bed to take a drink, fell back, and expired."

The affection of the nerves of the heart, which these cases illustrate, must be distinguished from another complication of diphtheria, in which structural changes take place in the heart itself. Dr. Bridger (*Med. Times and Gazette*), gives as the result of twenty-four *post mortem* examinations which he made, a roughened, reddened, and thickened appearance of the auriculo-ventricular valves, an affection which, he says, may be diagnosed by anxious countenance, hurried respiration, pulse 120 to 170, precordial tenderness, &c. He adds that there were about 100 heart cases out of 1,000.

Besides the various causes of sudden death now enumerated, suffocation may ensue, especially in young children, as M. Roger remarks, during attempts at swallowing.

We have indeed full confirmation of the opinion of Dr. Greenhow that "sudden death in patients the aspect of whose case is not alarming, or who appear out of danger, is a peculiar characteristic of diphtheria."

Dr. Carr of Blackheath, also says, "extreme suddenness of death is one of the most marked characteristics of true diphtheria."

Sudden death would be even more frequent after diphtheria than it is, were it not that the respiratory nerves have a power of retaining their vitality longer than others. "In persons under the influence of ether or chloroform, the power of the medulla oblongata to combine in action all the nerves of the respiratory muscles *outlives the irritation of the pharynx*, which, on irritation, will not produce movements of swallowing, or closure of the glottis, so that blood may run into the lungs or stomach."—*Ibid.*

The peculiar form of paralysis which has occupied so much of our space, is the only pathognomonic sequela of diphtheria. Other affections may, however, follow this disease, and may have more or less connexion with it or with the debility it causes. Two of our cases, a mother and daughter, suffered much from acute rheumatism; and Mr. John Bridger says, that diphtheria may be followed by pleurisy, pleuro-pneumonia, endocarditis, peritonitis, croup, abscess of the liver, gangrene of the lung, exudations on intestines and bladder, erysipelas of head and face, or of anus, umbilicus, penis, or vagina. Besides all these immediate effects of diphtheria, the general health may not be entirely restored for years. Such a case has lately been under my care, in consultation, with Dr. Atthill of Dublin, where a tendency to oft-recurring sore throat, with pellicular exudation and hemorrhage; as well as a formidable train of nervous phenomena, of a spasmodic character, at each catamenial period, seemed to arise from an attack of diphtheria six years before.

ART. XV.—*Case of Exophthalmos, caused by a Varicose Tumour of the Orbit.* By H. R. DE RICCI.

MANY causes may produce protrusion of the eye-ball. In early infancy it most commonly arises from malignant disease, and in such cases surgical interference offers but slight hope of success, though some rare cases are recorded in which complete extirpation of the eye-ball has succeeded in removing the entire disease, and effecting a permanent cure. Next in frequency, perhaps, may be noted pulsative aneurismal tumour; then different kinds of bony tumours, periostosis, hydatid tumours, dropsies of the capsule of Tenon, &c., &c. In the case, however, which I am going to relate the protrusion of the eye-ball was caused by a condition of parts such as I have never read of in any work of either general or special surgery, and not even alluded to in Demarquay's large work on *Tumours of the Orbit*—and as such I hope may prove of interest to my professional brethren. It was a very aggravated case of exophthalmos of the left eye, protruding fully three-quarters of an inch beyond the level of its fellow; projecting so much beyond the bridge of the nose that the eyelids could no longer be brought to close over the cornea. Although the protrusion was so great

that the optic nerve must have been enormously stretched, yet, strange to say, up to a few days before I operated, the sight continued wonderfully good, although, as might have been anticipated, the focal adjustment of the two eyes varied very considerably. I had been long watching this case, and had often examined the eye with great care, with a view to determining the exact cause of the exophthalmos. Following the most rational method of diagnosis I had eliminated or excluded, one by one, all the most unlikely causes, and thus malignant disease, aneurism, and periosteal tumour were successively set aside; still I could not bring myself to decide upon its exact nature. I thought at first that it might be dropsy of the capsule of Tenon, but the globe of the eye was unaltered in shape—it was only bulged out. Finally, having observed a considerable prominence pushing out both the upper and lower eyelid, on the outer side of the eye, I made up my mind that I had to deal with a hydatid sac, which, growing from the fundus of the orbit, protruded the eye-ball, and caused the two prominences which bulged out the upper and lower lid; and this view seemed to be confirmed by the fact that alternate pressure on the two prominences detected a feeling of fluctuation. I was wrong, however, in my diagnosis, as will be seen in the sequel, although I was correct as to the feeling of fluctuation. Finally, I determined on trying to remove the cause of the protrusion, whatever it might be, as soon as the sight would be seriously threatened, and before it should be irretrievably lost. The previous history of this case was as follows:—The patient, now aged twenty-two, is of rather strumous temperament, with very fair hair, and blue eyes; she always enjoyed good health, and never suffered the least pain or uneasiness in the affected eye. Sixteen years ago, when only six years of age, she was, by the neglect of a servant, allowed to fall from off a kitchen table on to the kitchen floor, striking the *right side* of her head against the stone flags. It must have been a severe blow, as the child was convulsed after the fall, and was some time before recovering. In a few days, however, the child was again, apparently, well, and it was only some considerable time after that strangers began to observe that the *left eye* was slightly more prominent than the right; the difference was, however, so slight that those persons which were immediately about the child did not observe it, and it was only after the lapse of three or four years that the parents perceived for themselves the protrusion of the child's eye, so slow had been its advance. Alarmed



at the discovery they had made, they consulted all the first medical and surgical authorities of that day; and when we reflect that Dublin could then boast of Cusack, Crampton, Marsh, Porter, Graves, and others, too many to enumerate, it will be readily conceded that my patient had the benefit of the best advice that Europe could offer. At this time, however, the eye protruded but little; a casual observer would not have noticed it; there was nothing to strike the observer; and the unanimous advice of all the eminent men who were consulted was to temporise, and watch what progress the exophthalmos would make. For several years the eye remained nearly stationary, its progress was so slow; but still it was progressing, and in the year 1859 Mr. Cusack was again consulted, when some local stimulating ointment, containing mercury and iodine, was directed to be rubbed in over the eye-lids, and about the orbital and temporal regions.

In 1860 the patient first came under my notice, though not under my care; and in a conversation with the late Sir Henry Marsh, who then had the management of the case, that sagacious physician remarked to me, "that eye will give trouble yet." At that time the protrusion was still slight, and by carefully viewing each side of the face in profile, I calculated it to be not more than an eighth of an inch in advance of its fellow. Things remained in this state for some time, the mercurial and iodized inunctions having apparently checked the immediate progress of the disease. This state of quiescence, however, lasted but a short time, and again the protrusion of the eye began to increase. A variety of remedies were now had recourse to; and though each in turn would seem to check the growth of the tumour for a season, still the protrusion kept steadily advancing. The patient now visited London, and there consulted most of the leading men. From what I could gather afterwards from the patient herself, their several opinions agreed in the main—the tumour, according to them, was not malignant, it was not an aneurism—most probably it was a cyst containing fluid; one surgeon offered to puncture the cyst and let the fluid out, but at the same time advised the patient to wait a little longer, especially as the sight still continued good, and there was no suffering. It was finally determined to wait a little longer before any operative interference was adopted, and to try in the meanwhile what could be effected by means of carefully applied pressure. Thus matters rested till the Autumn of 1864, when the patient came for the first time under my direct care. I had

not seen her for six months, and was much shocked at the greatly increased prominence of the eye. Looking at the face in profile from the left side, the cornea came fully on a level with the bridge of the nose, while on the right side a perpendicular tangent to the cornea would fall quite three eighths of an inch within the bridge of the nose. The case having now become mine, I studied it with increased interest, and assiduously applied myself to endeavour to arrive at a correct diagnosis of it. It was evident it could not be cancer in any form—it was too obvious the patient had never felt the slightest pain in it, and was the picture of health besides; it was not an aneurism by anastomosis, for there was no pulsation to be felt either by the patient or by the observer; it was not a case of dropsy of the capsule of Tenon, for the ball of the eye was perfectly normal in shape and free in its motions. Whatever the nature of it was, the tumour was situated in the fundus of the orbit, external to the muscular apparatus of the eye, bulging it forward, but not in any way interfering with its movements. After the patient had been a couple of months under my care, I observed a considerable bulging under the outer part of the upper eye-lid, about the region of the lachrymal gland, and after a few days a corresponding prominence of the external portion of the lower lid, and synchronously with these bulgings of the lids, the prominence of the eye became considerably greater; on placing the fingers over the upper and lower prominences of the eye-lids and making alternate pressure, a decided sense of fluctuation was detected, and the same feeling was obtained by keeping the fingers on those two points and making sudden pressure backwards on the ball of the eye. The eye itself was now becoming daily more prominent; there was no positive pain complained of, but there was considerable uneasiness in it; if now it was seen in profile from the left side, it was observed to be considerably in advance of the bridge of the nose—so much so that the pupil and iris could be distinctly seen even when the face was viewed in profile from the right side; the protrusion had evidently increased as much during the last three months as it had during the previous six years; if it should continue to come forward at this rate the eye would be out on the cheek beyond the grasp of the eye-lids in a few weeks at furthest—the sight was becoming rapidly worse, and the patient was getting anxious for more determined measures. Under these circumstances I determined on an operation, and with the kind and able assistance of my friend Mr. Hutton, proceeded as follows:—The patient

having declined to inhale chloroform, I placed her in the recumbent position, and at once made an incision, about an inch and a quarter in length, dividing the lower lid parallel to the margin of the orbit; I then carefully divided the orbicularis muscle in the direction of its fibre, to an equal extent with the first incision; I then came on a quantity of fat traversed in all directions by tense shining fibrous bands, like threads, which I carefully broke down and removed. At this stage of the operation there appeared at the bottom of the wound a bright, tense, shining membrane, having all the appearance of a hydatid cyst; and, as that had been my diagnosis of the case, I was congratulating myself silently on my correctness, and had begun carefully to separate the supposed cyst from the surrounding structures, when it suddenly gave way, and a large quantity of a semi-fluid granular fat was poured out. The eye being very prominent it was easily displaced towards the bridge of the nose, and thus sufficient space was afforded for the introduction into the orbit of one finger, with which I was enabled to reconnoitre the parts; here I again found the orbit traversed in every direction by thin, but firm, tendinous bands, like threads, which I carefully divided with a small probe-pointed knife, and having evacuated a further quantity of loose adipose matter, I took the protruded eye in my left hand, and making pressure with it backwards, endeavoured to replace it in its right position; during this manœuvre, however, I dislocated the real tumour, which had been the cause of this terrible exophthalmos, for as I was endeavouring to replace the eye with my left hand, the index finger of my right hand which was still in the outer portion of the orbit came suddenly in contact with a round soft substance, and on withdrawing my finger and pressing still further back the eye, a round lobulated purple tumour, the size of a cherry, sprang up into view; its look, its feel, were unmistakable; it was undoubtedly a venous tumour; but to make sure, I punctured it, when a considerable amount of a brown watery fluid escaped, followed by active venous hemorrhage. I lost no time in passing a ligature round the tumour, and having obtained a good hold of it, I made firm traction on the ligature with my left hand, while with the index of my right I endeavoured to separate the tumour, as much as prudence would allow, from its deep connexions, with a view to tying it as near to its origin as possible; I then handed the ligature to Mr. Hutton, and while he made firm traction of the tumour I passed a second ligature round it as far back as I could reach, and cut away the tumour in front

of it. I then replaced the eye in its normal position, the patient was put to bed, and a simple pledget of lint soaked in iced water was applied for dressing.

Nothing could have exceeded the satisfaction of both doctor and patient, when, a few days after the operation, the eye was found, on examination, to be not only in its right place, but to have wonderfully recovered its sight. One month after the operation the lady was out, and well; the eye was in its place, and the sight totally restored, and all the movements of the eye-ball perfect. This happy state of things lasted, however, only a short time. Three months had scarcely passed over when the eye again began to protrude. Strong pressure and mercurial application were again tried, but to no purpose. I then advised that the same operation should be attempted over again as soon as the sight should be in danger; and if, during this second operation it should be found impossible to separate the tumour completely from its attachments I recommended a complete extirpation of the eye-ball, saving as much of the muscular structure as possible, for the purpose of fitting an artificial eye. This was terrible news for the poor patient; and her friends, naturally wishing for the best advice before consenting to the total loss of the eye, resolved on consulting Dr. Bowman, to whom I gave them a letter. He took exactly the same view of the case, and having obtained the lady's permission for the total ablation of the organ, should he find it impossible to remove the tumour without, proceeded to operate (this time under chloroform). The operation, as Dr. Bowman wrote to me, was extremely long and tedious; but he tried in vain to separate the tumour from the structures in the orbit, and, finally, was compelled not only to remove the eye-ball but all the muscles also, so thoroughly matted together were all the parts engaged. The convalescence was rather tedious, but up to this there has been no return of the disease. The tumour was examined by Dr. Bowman, who found it, as I had also done, a mass of convolutions of veins dilated into sacculi, these being traversed by tendinous threads, like the chordæ tendineæ of the heart.



ART. XVI.—*Some Remarks on the Nature and Treatment of Pulsating Thyroid Gland with Exophthalmos* ("Graves' Disease"). By WILLIAM MOORE, M.D., Dub.; M.R.I.A.; Fellow of the King and Queen's College of Physicians; Physician to Mercer's Hospital; Physician in Ordinary to Sir P. Dun's Hospital, and to the Institution for Diseases of Children; Lecturer on Practice of Medicine in the Ledwich School of Medicine, &c.

THE deep interest which attaches to this distressing affection, the fact of its comparatively unfrequent occurrence, and of the subject not being hackneyed, induces me to contribute the following brief observations to the pages of this valuable Journal. The affection now generally termed "Graves' Disease," viz., palpitation with visible pulsation in the carotids and thyroid body, with exophthalmos, has been treated of more or less, by writers at home and abroad. At home mainly by Graves,<sup>a</sup> Stokes,<sup>b</sup> Marsh,<sup>c</sup> Banks,<sup>d</sup> Begbie,<sup>e</sup> Mackenzie,<sup>f</sup> and Laycock;<sup>g</sup> whilst the foreign contributors are Withusen,<sup>h</sup> Græfe,<sup>i</sup> Desmarres,<sup>j</sup> Arlt,<sup>k</sup> Helft,<sup>l</sup> Bruck,<sup>m</sup> Basedow,<sup>n</sup> Trousseau, and others. Among the most remarkable phenomena of this affection are the cardiac pulsations, which exceed in frequency and violence those commonly met with in organic disease of the heart. They are so rapid and tempestuous in some cases it is difficult to define their frequency. The carotids and arteries of the upper extremities partake of this excessive vascular excitement, whilst the circulation in the lower extremities is below par. Occasionally the external jugular veins become prominent, and borrow pulsation from the adjacent vessels, whilst the thyroid gland becomes enlarged, pulsates, and conveys *fremissement* to the hand, a musical murmur being occasionally audible. Next comes the exophthalmia, the eyes (or eye, as has been once or twice observed) becoming prominent, in some cases painful on being handled, and accompanied with imperfect vision. Superadded to these objective symptoms we meet with mental depression in

<sup>a</sup> Clinical Medicine.

<sup>b</sup> Diseases of the Heart and Aorta.

<sup>c</sup> Dub. Journal.

<sup>d</sup> Dub. Hosp. Gazette, 1855.

<sup>e</sup> Edinburgh Med. Journal, 1863.

<sup>f</sup> Diseases of the Eye.

<sup>g</sup> Edinburgh Med. Journal.

<sup>h</sup> Bibliothek fur Læger.

<sup>i</sup> Archiv. iii.

<sup>j</sup> Gazette des Hôpitaux.

<sup>k</sup> Krankheiten des Auges.

<sup>l</sup> Casper's Wochenschrift, 1849.

<sup>m</sup> Heidelberger Medicinische Annalen.

<sup>n</sup> Med. Times and Gazette, 1862.

various aspects, globus, vertigo, tinnitus, and other such distressing signs.

Bruck and Basedow found in some of their patients a hysterical condition, with morbid craving for amusement and dissipation, but such has not been my experience.

With respect to the exciting cause of this malady, among the six cases which form the subject of this paper, of which five were females and one male—in three of the female cases the occasion of the disease was attributed to shocks from sudden deaths or mental emotion with subsequent depression; in another female case profuse menorrhagia; and in the fifth, loss of independence and social position seemed the origin of the affection; whilst in the instance of the male, the most rational explanation of the occurrence of the disease was excessive sexual indulgence.

Withusen (whose valuable paper on this subject has been translated by Dr. W. D. Moore, of this city, in the *Dublin Medical Press*, Vol. XLII.) tells us that the disease has been referred to causes which have had a depressing influence on mind or body, such as long-continued or very violent mental affections, from over application to study, still more frequently to weakening discharges—as hemorrhage, diarrhea, excessive lactation or leucorrhea.

The following details of a case under my observation will exemplify the symptoms and physical signs of this distressing malady:—Mrs. C—, aged thirty-four, a slight spare woman, consulted me about three years ago for general nervous and hysterical affections, palpitation and globus being chiefly complained of. On further inquiry I found she had been suffering from profuse menorrhagia for the previous eighteen months, that for the six months before my seeing her she remarked her neck becoming thick and swollen, and that at times she was afflicted with violent throbbing in her head. These symptoms abated from time to time, but on their recurrence they always seemed aggravated, till the eyes became staring and restless, and the catamenia, instead of being excessive, became deficient in quantity and colourless, the subjective symptoms keeping pace, more especially the complaint of suffocation, and painful throbbing along the neck and each side of the head. The heart's action at this stage of the disease was over 140 in the minute, and this excessive cardiac pulsation was visible, and could be heard along the great vessels, accompanied with a loud souffle. The thyroid gland was enlarged, the right lobe may be said to have increased a little, it was certainly larger than

the left, and distinct pulsation, with *fremissement*, could be felt all over it; the patient was also suffering from partial aphonia. The exophthalmia was most remarkable, the pupils were dilated, and the eye generally restless and fidgetty. The sclerotic coat of a muddy yellow hue, whilst the patient complained of dimness and partial loss of vision, with pain on the eye being touched or handled.

This case gives a general view of the symptoms and physical signs which this affection presents. In the other cases which I carefully observed there were some variations. For instance, in the case of a tall handsome woman, about thirty years of age, the pulsations became so frequent, irregular, and tempestuous, that it was impossible to determine their frequency with accuracy, whilst the pulsating carotids stood out as thick as ordinary sized middle fingers. The thyroid body was enlarged and pulsated, but it did not attain a great size; the right lobe, however, was larger than the left. There was no evidence of spanemia about this patient; but as the disease progressed there was manifest diminution in the circulation inferiorly, and the menstrual function, which had been irregular from the commencement, eventually became suppressed.

This case proved fatal, with all the symptoms of valvular patency, occasional delusions having occurred before death.

The *third* case was a young woman about twenty-seven years of age, florid and muscular. In this case, the exophthalmos was very remarkable, and the eyeballs painful on being touched; the right half of the thyroid gland was larger than the left, and the cardiac pulsations exceeded 150 in the minute; the catamenia were deficient, but the improvement in the patient took place, *pari passu*, with the menstrual restoration.

In the *fourth* case that of a young married woman, staring disfiguring exophthalmia, imperfect vision, and general spanemia, with thyroid enlargement and palpitation, were the prominent symptoms, all of which seemed to abate after the occurrence of pregnancy.

The *fifth* case occurred in a married woman, 30 years of age, who had never borne children, and who suffered from total suppression of the menses, the exophthalmia in this case was revolting; the right half of the thyroid gland was larger than the left, and there was well nigh complete aphonia. In this case also the general improvement in the other symptoms was coeval with the restoration of the catamenia.

The last case I shall here adduce was that of a deaf mute, aged

thirty-one, who presented this affection in an aggravated form, but which yielded readily to free doses of digitalis. Of these six cases only one has proved fatal, with œdema, ascites, and all the other symptoms and signs of valvular insufficiency and general cardiac dilatation. Such a condition corresponds with the pathology of this disease as described by Sir H. Marsh and others. In Sir Henry's case both auricles, particularly the left, were dilated and hypertrophied, and the auriculo-ventricular valves of both sides exhibited thickened margins. In Professor Smith's case there was hypertrophy and dilatation of the left ventricle.<sup>a</sup> Dr. Banks<sup>b</sup> found the thyroid gland enlarged to four or five times its natural size, the right lobe larger than the left, and the body solid and lobulated, a section disclosing numerous cysts. The heart was as large as that of an ordinary sized man (the patient being a woman under the middle size); its valves were free from disease, with the exception of slight thickening of the anterior edge of the mitral valve. The lungs were congested, the liver appeared in an early stage of cirrhosis, the spleen was large and congested, the brain softer than natural, the kidneys had undergone the changes usually observed in the early stage of Bright's disease, albumen having been present in the urine during life, whilst hypertrophy of the left ventricle, without dilatation, existed; in addition a thickened, rigid, uneven condition of the semilunar valves. The right side of the heart was dilated without hypertrophy; much fat was found in the heart, and atheromatous deposit in the aorta and its branches—even in the arteries of the brain, the ophthalmic, and ciliary. The thyroid body was enlarged and hypertrophied, its vessels dilated and tortuous, and in some cases aneurismatically.

Cushions of fat have been found, in some cases, behind the eyes, which generally present the appearance of deep-seated inflammation.

In the case of a woman who died of apoplexy in the Hotel Dieu,<sup>c</sup> Professor Trousseau found the superior part of the sympathetic system in a state of marked congestion; the texture of the ganglia

<sup>a</sup> Hensinger found hypertrophy and dilatation of the left ventricle with great flaccidity of the muscular substance, whilst Prael met with the same condition of the left side of the heart, with atheromatous degeneration engaging the endocardium and aortic arch, with extensive softening of the brain. Casper Wochenschrift, 1851. Newman reports a case in which vision was lost several months before death. Deutsche Klinik, 1853.

<sup>b</sup> Dublin Hospital Gazette, 1855.

<sup>c</sup> Journal of Practical Medicine and Surgery, 1864.



had undergone alteration, a portion of their substance being replaced by cellular tissue; and in Dr. Cruise's case, the *post mortem* examination of which he made most carefully, with the assistance of Dr. Robert M'Donnell, the inferior cervical sympathetic ganglia were almost obliterated, being supplanted by cellular and adipose tissue.

These latter observations on the nervous pathology of this affection are most important; and I have no doubt when further extended to the nerves of the cord in general, and those of the cilio-spinal region in particular, increased light will be thrown on the nature of this interesting disease.

With respect to the enlargement of the thyroid body, which occurred in these cases I have detailed, it did not attain the size of ordinary goître. Some have asserted that there is no observable difference between this thyroid enlargement and that of ordinary bronchocele; but such is not my belief; in none of the above cases did the tumour attain a very great size, and after a time it became stationary, the right lobe being larger than the left, and in most instances the thyroid enlargement sensibly decreasing, *pari passu*, with the decrease in the palpitation; of course where the disease is of long standing, and the gland has become thickened and hypertrophied from a varicose state of its vessels, such a favourable condition of things is not to be expected. Still the fact of the decrease of the thyroid body keeping pace with the diminution in the cardiac pulsations goes far, in my mind, to refute M. Piorry's theory, viz., that the thyroid enlargement is the starting point from which the other symptoms result—the pressure from the increased gland causing dyspnea, cardiac embarrassment, and exophthalmia.

But the question naturally suggests itself, to what are all these formidable symptoms due and owing? and on this point there is a great diversity of opinion.

Dr. W. Begbie holds that the true pathology of the exophthalmos and enlarged thyroid body, found associated with cardiac palpitation and vascular pulsations, is due both to the blood and nervous system, but that the condition of the circulating fluid is the main agent, viz.: that an altered state of the blood, stopping short of what is generally termed anemia (but in many cases amounting to this condition) acts directly on the nerves of the blood-vessels and on the cardiac nerves; that as a sequence their movements are seriously affected, the results being—dilatation of blood-vessels, arteries, and veins, and of the chambers of the heart itself. He

believes that for a long period the bronchocele is, or may be, vascular enlargement and dilatation, but in course of time hypertrophy and structural degeneration of the gland ensues. The exophthalmos depending upon vascular congestion and dilatation of the ophthalmic vessels with effusion of serum into the post orbital cellular tissue. Lastly, he concludes that a plan of treatment, with a view to an improvement in the condition of the blood, and simultaneously in the state of the nervous system, is successful in effecting a cure, provided radical structural changes have not taken place.

Trousseau, on the contrary, does not consider "Graves' disease" a cachexia, as chlorosis, or albuminuria, and he considers the co-existence of anemia as only consecutive to the disturbance of nutrition, albuminuria being a very rare symptom of exophthalmic bronchocele. He believes the disease to be a neurosis, somewhat analogous to hysteria, characterized by local determinations, having its origin in some modification of the vaso-motor (sympathetic) system. The palpitation, enlargement, and pulsation of the thyroid body, and the exophthalmos are direct consequences of this; and the diarrhea, the diuresis, and diaphoresis are due to congestion of the glandular apparatus—the irregularity of the menstrual function being caused by the deficiency of blood in the utero-ovarian system; and if this is restored, either by pregnancy or other causes, many of the symptoms of the disease disappear, as if the return of the uterine hyperemia caused the morbid congestions of the other organs to cease. Dr. Laycock attributes this affection to nervous agency, whilst Dr. Stokes has not found this disease associated with any form of carditis, and concludes that it is a special form of cardiac neurosis which may eventuate in organic disease. Whether the nervous excitement is propagated to the arteries in the neck is a question he has often proposed to himself, for there is something in their action which he considers cannot be altogether explained by the force of the heart. The double pulsation of the arteries of the neck, and those alone, he considers another evidence of local vascular excitement. Both Hensch and Stokes agree that this condition is not attended with fever, nor with physical signs or general symptoms of cardiac inflammation.

Now, although many of the most prominent symptoms of this affection, as palpitation, menstrual suppression, and mental anxiety are compatible with anemia, still I am not prepared to admit such to be the exciting cause of this disease. In four of the cases I have

adduced the appearance of the patients was not spanemic; on the contrary, three of them might be called florid and well developed, and when ferrugineous preparations were exhibited they proved ineffectual.

In this view of the question I am not singular; for whilst Withusen admits that in a great proportion of cases there was well marked anemia, with suppression of the menses, and cardiac symptoms such as would suggest the inference that this morbid condition stands in direct relation to the phenomena of Graves' disease; still, he goes on to say—if this blood disease had always evinced itself in a greater or less degree we should have progressed in our knowledge of this affection; but such is by no means the case, as numerous instances could be shown where not a trace of anemia was present; on the contrary, the patients were blooming, active, and plethoric, and where the remedies usually employed in anemia were found absolutely useless. Nor does Henoch regard anemia as an indispensable element in the development of the disease.

For my own part I consider the origin of this affection to be essentially nervous, in which the vaso-motor system plays the most conspicuous part. When we consider that the motor fibrils of the heart are derived from the sympathetic as well as from the spinal nerves; and as Professor Moleschott has shown that the heart is animated by four very excitable nerves—two vagi and two sympathetic, having a peculiar consensus, so that the state of irritation or over-excitement which is produced in one of the nerves is transmitted to the other three; again, branches from the inferior cervical sympathetic supply the thyroid body and larynx; such being the nervous distribution we may easily reconcile palpitation, pulsation of the carotids and thyroid body, with any vaso-motor irritation along this important region. Then comes the question of exophthalmia, dilated and restless pupils, &c. It is now an admitted fact that there are two antagonistic set of fibres in the iris—the circular or contracting fibres surrounding the pupil being influenced by the third cranial nerve, whilst the dilating fibres of the pupil are influenced by the sympathetic.<sup>a</sup> Drs. Budge and Waller, however, have shown that the fibres going to dilate the pupil originate from the spinal cord, from the region termed the “cilio-spinal,” which extends from the sixth cervical to the fourth dorsal; and Brown-Séguard gives even a more extensive origin to

<sup>a</sup> Vierordt's *Archiv. Fur Physiol. Heilkunde*, 1852.

these; and with respect to the vaso-motor influence of these nerves, he has discovered that it is derived chiefly from the spinal cord, by the last cervical and two highest dorsal nerves. Schiff<sup>a</sup> goes still further, maintaining that the dilating fibres of the iris originate from the highest cervical nerves, even from the medulla oblongata. Under any circumstances any excitation employed along this cervico-dorsal region will cause increased vaso-motor action—hence exophthalmos and dilatation of the pupil. But it may be urged might not this dilatation of the pupils be attributable to some deficiency in the nervous action of the third pair, which would lessen the normal counteracting contractile action of the iris, and hence dilatation. This, I think, can readily be met by the fact that there is no evidence of disease of the brain or symptoms of compression. In short the conclusion most satisfactorily arrived at in my mind is, that the cardiac pulsations, pulsating thyroid gland, and carotids, with exophthalmos, dilated and restless pupils, are due to the increased vaso-motor or sympathetic action. Further, the experiments of Schiff, Bernard, and others go to prove that the disease of the sympathetic system induces in the organs more immediately under its influence changes which eventuate in atrophy; the texture of the various organs, from impaired nutrition, being replaced by adipose and cellular tissue. Such being the condition these cases usually present. Again, when we turn to the condition of the lower extremities, we find this abnormal vascular excitement superiorly starving the lower extremities of their due supply of blood, and hence the menstrual irregularities which ensue; the restoration of this function to as nearly a healthy standard as possible being a sure indication of general amendment. Withusen, however, mentions one case in which menstruation was checked for two Summers; on the first occasion it was absent for five months without any apparent change in the patient's state, while on the last occasion the visible improvement in the case coincided with the arrest of the function. Such an instance has not come under my observation, but in one of my cases pregnancy seemed to exert a favourable influence. With respect to the prognosis of this disease, the more I see of it the more hopefully I am inclined to regard it. That the affection has proved fatal more rapidly in men than in woman is generally admitted; but this I cannot endorse from my own experience, as all the cases I have

<sup>a</sup> Untersuchung z Physiologie des Nervensystems.



seen occurred in females, with one exception, and as yet, as far as I can learn, only one of these cases has proved fatal. In some of the others perfect recovery seems to have taken place, whilst in one case, which I have frequent opportunities of observing, the cardiac distress, carotid and thyroid pulsations have ceased, but the gland still remains enlarged, with partial exophthalmos.

The character of the affection, without doubt, is very prone to relapses, which sooner or later must entail organic changes incompatible with length of life; still with common care, and due prophylactic means being adopted, these relapses may, to a great extent, be anticipated. Among the most important of these means I should consider the distraction of the patient's attention as far as possible from what may have proved, in his or her case, the special exciting cause, for which change of scene and society will be found indispensable. As regards the special treatment of the disease, bleeding is inadmissible; but Withusen has found the application of a leech or leeches to the external cavities of the eye useful where there was much pain and tension associated with the exophthalmos. Of the therapeutical remedies I have found the tincture of digitalis, in doses of from fifteen to twenty-five drops—three or four times a-day, most efficacious—combining it with iron where I had reason to believe spanemia existed. The extract of digitalis may also be applied locally over the precordial region. Bromide of potassium I look upon as a valuable agent in these cases, but I should give it in much larger doses than those usually directed in books—say from fifteen grains to half a drachm, three or four times daily; its special therapeutical properties I am not prepared to define, but it seems to me to exert a sedative and equalizing effect on the vaso-motor system generally, more especially in restoring the uterine functions to their normal condition. The use of iodides, both internally and externally, have proved equally inefficacious in my hands. Yet Professor Trousseau inclines to the belief that iodine, internally and externally, combined with hydropathy, will, perhaps, be found the most useful resource of the therapist in this singular disease. Iron, *per se*, has also disappointed me. I think in these cases it would be well to keep up counter-irritation along the cilio-spinal region either by means of small blisters or liniments, such as the compound camphor or soap liniments, with belladonna or chloroform.

ART. XVII.—*Two Cases of Onychomycosis, with Remarks.* By JOHN M. PURSER, M.B., T.C.D.; L.R.C.S.I.; Demonstrator of Anatomy in the Carmichael School of Medicine.

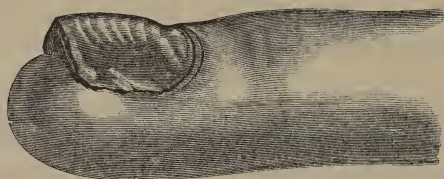
AMONG the various "new diseases" brought to light of late years, by our improved means of diagnosis, not the least remarkable are those characterized by the presence, whether as cause or effect, of vegetable growths on the skin and mucous membranes; but, while parasitic disease, as it affects the general integument, and more particularly those parts which are covered with hair, is now well understood, there is reason to suppose that very frequently fungi luxuriate beneath the nails without their presence being suspected, and that much of the obstinacy of diseases of the nails is due to this fact. Instances, indeed, are not wanting where diseased nails accompanied disease of other parts, and the existence of a fungus was suspected or proved; but, with the exception of those published by Meissner and Virchow, there are few cases on record where parasitic disease of the nails existed alone. It is for this reason that I have been induced to publish the two following cases which have recently occurred to me, although, except from a diagnostic point of view, they present but little interest.

A young lady, in the habitual enjoyment of good health, consulted me for a slight indisposition, and, at the same time, showed me her left thumb, the nail of which presented the following appearances:—It was of a dirty brownish-yellow colour, streaked with lines of a darker brown, greatly thickened, and at its free extremity separated from its bed by a mass of soft nail substance which could be easily picked out. The entire nail was somewhat roof-shaped, a prominent ridge running along its centre, from which it sloped down on each side towards its attached edge. Its sides were concave from above downwards; its surface was very rough, and marked by deep transverse grooves; the longitudinal striæ also were strongly marked; it was very hard, more brittle than natural, and inclined to split longitudinally and in flakes. Near the root was a small portion of nail of a pink colour, but rough and thickened; there was no trace of lunula; a small abscess existed at the root, and the skin in the neighbourhood was slightly red and swollen. The annexed woodcut (Fig. 1), from a drawing by Mr. Connolly, gives a tolerably good representation of the nail.

The history of the case was briefly as follows:—About three

years ago the patient suffered from a cutaneous affection of uncertain

Fig. 1.



nature which was confined to the dorsal surface of the left thumb; the skin became red, and little blisters formed. She consulted a surgeon, and, under treatment, the part recovered itself in a few days. Shortly after this she perceived the nail, at its upper part, becoming discoloured and thickened. The discolouration and thickening extended with the growth of the nail from above downwards, and have been increasing ever since. With the exception of the deformity caused by it the patient has suffered no inconvenience from her disease till lately, when she became subject to small collections of matter, which form under the nail near its root, and either discharge themselves by the edge of the nail or are absorbed, leaving small yellow spots to mark their former site. She has undergone great variety of treatment, including repeated blistering of the skin around the nail, but without benefit. The other nails are all healthy, and, with the exception of that already referred to, the patient has never suffered from any skin affection.

A portion of the nail and some of the loose substance lying under it were removed, and submitted to microscopic examination. The superficial horny part of the nail, with the exception of its thickness, presented nothing very remarkable. Some of the cells, however, were opaque and granular, and others were of a brownish colour; but in the deeper layers of the nail the elements of a fungus growth were found in great abundance.

These were:—I.—*Spores*, circular or oval, either scattered, collected in groups, or forming moniliform chains. In some of them a central nucleus-like spot was apparent. II.—*Tubular filaments*, tortuous, and branching; these were for the most part jointed at intervals (*d*), and many of them contained small shining bodies (*e*). III.—*Larger, less branched, filaments*, of brownish colour, and containing spores at regular and close intervals (*b*); the walls of these filaments were sometimes indistinct, the spores being apparently attached to each other, end to end, forming a moniliform chain (*c*), which was often seen to terminate in a dense cluster of minute

spores, or in a mass of granular matter (a). IV.—Granular matter.

Fig. 2.



All these were mixed up with tolerably healthy nail plates, and were rendered very clear by caustic soda or potash.

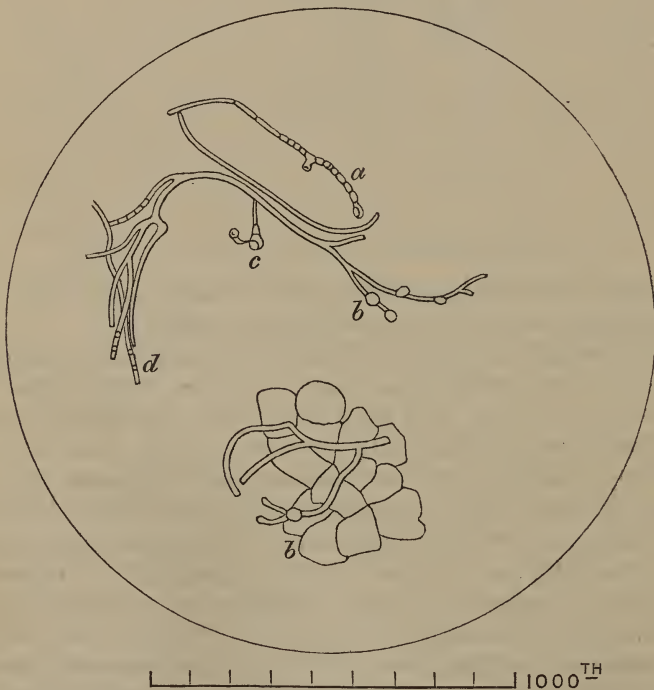
In the second case the nail disease was discovered by accident. The patient was a middle-aged man, who in his younger days had suffered severely from both gout and syphilis, but who for many years had been free from both disorders. At the time he came under my observation he was labouring under advanced Bright's disease, the urine being highly albuminous, and loaded with fatty casts and degenerated renal epithelium. All the nails of his fingers were remarkably curved, presenting the appearance so frequently seen in chronic pulmonary disease; otherwise they presented nothing abnormal, with the exception of the nail of the right index finger. This was of a brownish yellow colour, with darker spots and streaks, without polish, and having the longitudinal striæ strongly marked. It was not so much thickened as in the first case, nor was it so



much separated from its bed—the loose growth of cells and fungus beneath it being less abundant. It was very friable, and inclined to split both longitudinally and transversely. In this nail, also, there was no trace of lunula, but a small part of a pinkish-yellow colour existed at its attached end. The extent of this pinkness varied at different times. With the exception of a secondary syphilitic eruption, this man had never had any skin affection, nor did he remember ever to have received any injury to which the nail disease could be attributed. The discolouration commenced at the root and grew downwards. The alteration dates back about five years, and has never given him any inconvenience. The nails of the great toe and of the second toe of the right foot presented appearances somewhat similar to those of the finger. All the other toe nails were healthy.

The microscopic appearances (Fig. 3.) in this differed slightly from those seen in the former case.

Fig. 3.



I. *The spores* were for the most part oval, and very minute; they

were either scattered over the field or formed groups; the latter arrangement, however, was less common than in the other case. In some instances a spore appeared to be germinating, a small projection extending from one extremity; in others a further stage of this germinating process was observed, two spores cohering end to end, resembling the appearances seen in the growth of torula; and in others moniliform chains were formed, one of which terminated in an enlarged oval cell, containing a shining nucleus-like body (*a*). The termination of these chains in clusters of spores was not observed in this case. II.—*Tubular filaments*, variously ramified and branching, sometimes jointed, but in many instances presenting no articulation, either empty or containing at varying intervals minute shining bodies (*d*). In some cases the course of a tube was interrupted by a round celloid body, from the opposite sides of which passed off its ramifications (*b*). In one specimen an appearance somewhat resembling that seen in puccinia was noticed, a tube terminating in a pear-shaped enlargement divided into two segments, of which the distal enclosed an oval body (*c*). III.—*Granular matter*.—In considering any case of so-called epiphytic disease the question suggests itself, whether the bodies found in the diseased spots are really vegetable growths or the results of perverted development in the cells of the part. Those who hold the latter view are now decidedly in the minority, but they number among them Mr. Erasmus Wilson, whose opinion on all matters connected with diseases of the skin is of the greatest weight. This writer believes that “favous matter, and the mucedinales of the phytodermata, are organic matter arrested in development at the lowest degree of life, the function of reproduction; the sporules are growing organic substance, aborted epidermic granules; the filamentary portion fully formed organic substance, beyond which there is no further growth, the highest and perfected form of development.”<sup>a</sup> But the reasons given by Professor Bennett,<sup>b</sup> for considering these structures to be really vegetable organisms, distinct from the body on which they grow, supported as they are by the opinions not only of pathologists, but of professed botanists, appear quite unanswerable, and we have little doubt that in a short time this will be the only opinion held. But granting the truly parasitic nature of these growths, a more

<sup>a</sup> British and Foreign Medico-Chirurgical Review, January, 1864, p. 204. In this paper, besides the view of Mr. Wilson, will be found an admirable *résumé* of the opinions of M. Bazin, the great champion of the opposite school.

<sup>b</sup> Principles and Practice of Medicine, 2nd Edit., p. 802.

difficult question arises, namely, are they cause or effect of the disease which they accompany; are they capable of germinating on a perfectly healthy body, or do they grow because the part already diseased presents to them a suitable soil. The generally received opinion is, that all persons are not alike prone to epiphytic disease, but that certain conditions, as youth, dirtiness, weakness, and above all the existence of non-specific eruptions, are necessary in order that the fungous germs falling on the skin may come to perfection. This is well shown in an experiment of Bernard:—"When frogs have been kept long in captivity, their health declines, and ulcerations arise around the mouth and nose; the nervous system being in this case considerably depressed, the animal is of course found to resist much longer the action of strychnia and similar poisons, while parasitical affections spread with fearful rapidity. Frogs are subject to the growth of parasitical fungi, which after a certain lapse of time, occasion the animals death. Now, if a healthy frog be placed in a jar containing others affected with the above-mentioned disease, the new comer sets contagion at defiance; while if another frog affected with ulcerations in the vicinity of the natural orifices is introduced into the jar the parasitical vegetation covers it at once."<sup>a</sup> The parasite, however, once having taken root, becomes the principal cause of the local disease which it serves to keep up and aggravate, and its destruction must be undertaken at an early stage of the treatment, while at the same time an endeavour is made by general means to render the soil no longer fit for its growth.

We have lately been assured, with some vehemence, by Dr. Cobbold that nothing can be more contrary to the teachings of science than to suppose that an enfeebled state of body is advantageous to the invasion of parasites.<sup>b</sup> What Dr. Cobbold says of animal parasites, if it be true, may be extended to those of vegetable nature. It is, however, an opinion not likely to find much favour with practical physicians, who look on parasitic diseases more from a clinical than from a natural historical point of view, and more from the side of the patient than from that of the parasite. Indeed, if this were so, there are few of us whose intestines would not harbour *tæniæ*, and in whose hair follicles the *achorion* would not flourish, for no one has shown more ably than Dr. Cobbold himself the facility with which parasitic germs find access to the human body.

<sup>a</sup> Brit. and For. Medico-chiurgical Review, April, 1864, p. 386.

<sup>b</sup> Lancet, Vol. i., 1865, p. 326.

In both cases, recorded above, there is, I think, reason to believe that the fungus growth was secondary to disease of a non-specific kind. This is shown by the alteration in the nail having commenced at the root and extended downwards. In the first case it is possible that the previous cutaneous disease may have exerted a morbid influence on the secreting structures at the root of the nail; and in the second case the original disease may have been of syphilitic origin, or have been caused by injury, as appears to have been the case in an example of nail fungus recorded by Meissner.<sup>a</sup>

The doctrine of spontaneous generation meets with but little support at present, and deserves even less than it receives, so that the origin of a parasite must always be sought outside the patient on whom it lives. With regard to my first patient I learned that about the time of the commencement of her disease a dog, to which she was much attached, was suffering from a skin affection characterized by falling off of the hair in circular patches, and doubtless parasitic; and though the patient herself is convinced to the contrary, yet it is the opinion of some of the members of her family that she contracted the disease from the dog; of course, any proof of this theory is impossible, but as the patient, from her position in life, was unlikely to come in contact with persons affected by any of the usual forms of epiphytic disease, and as the transmission of parasitic germs from men to animals, and *vice versâ*,<sup>b</sup> has been shown to be by no means unusual, it seems that this view of the origin of the disease is far from improbable. In the second case nothing could be learned by questioning the patient; but the fact of his having worked for many years in a brewery is worthy of note, for Dr. Lowe has found epiphytic disease with great frequency among brewers, and attributes it to contact with the growing yeast. I may mention, however, that with unusual opportunities for observing the diseases of brewery labourers I have been unable to confirm the observations of Dr. Lowe. Skin affections, indeed, are common enough among them, mostly of an eczematous type, and assuming, almost invariably, the form described by Bazin as *arthritic*,<sup>c</sup> characterized by want of symmetry, tendency to remain localized, scantiness of discharge, and, in many cases by intense itching, their true

<sup>a</sup> Küchenmeister—Manual of Parasites (Sydenham Society), Vol. ii., p. 228.

<sup>b</sup> *Von Bärensprung*.—Brit. and For. Medico-Chirurgical Review, July, 1857, p. 263.  
Bazin.—Leçons sur les Affections Cutanées Parasitaires. Deuxième édition, p. 126.  
Frazer.—Dublin Quarterly Journal, Vol. xxxix., p. 294.

<sup>c</sup> Leçons sur les Affections Cutanées de Nature Arthritique et Dartreuse, p. 183.



nature being further elucidated by the co-existence or alternation with them of unmistakable gouty or rheumatic symptoms; thus presenting a marked contrast to the same affections as they occur in the children of these men, in whom they generally present the *scrofulous* character marked by little or no itching, profuse discharge tending to become purulent, and enlargement of the lymphatic glands connected with the diseased part. However, as we have already seen, when considering the microscopic appearances of the fungus, in this case, some of the appearances of torula were present; and whether we agree with Virchow in classifying the nail fungus with aspergillus, or, with Küchenmeister, refer it to achorion or oïdium; its relation with the yeast plant is not very distant, for Dr. Fox<sup>a</sup> has traced the transitions from torula to aspergillus, and has shown that torula in its growth may produce the fructification of oïdium, while Mr. Hogg believes that torula and achorion are identical. The peculiar appearances seen at *b* (Fig. 3) resemble those sometimes seen in the germination of achorion, while at *a* the similarity to oïdium is marked.<sup>b</sup> In the first case the filaments crowned, as it were, by clusters of spores bore some resemblance to aspergillus; but, on the whole, the appearances were rather those of achorion or oïdium. The classification of fungi, at all times a difficult and uncertain study, has become of peculiarly little interest now, since from the investigations of Hebra, Lowe, Fox, and others, we have good reason to believe that the apparently different fungi, formerly classed as so many distinct species, are, in reality, but different modifications of the same plant, the diversity in appearance being due to differences in seat, stage of growth, and external influences.

With regard to treatment I have but little to say. In neither instance was I originally consulted about the nail disease, and the state of health of the man, the subject of the second case would render any interference unjustifiable. Quite recently I have been requested by the lady, whose case was first recorded, to do something to remedy the deformity of her thumb, which annoys her very much. Believing, as I do, that in all cases of epiphytic disease the destruction of the fungus should be the first step in the treatment, I cut and scraped away as much of the thickness of the nail as I could without giving pain, and to the remainder I applied

<sup>a</sup> Skin Diseases of Parasitic Origin, p. 151-164.

<sup>b</sup> See Robin—Histoire Naturelle des Végétaux Parasites, Pl. i., Fig. 5 K, and Pl. ii., Fig. 13.

freely, as a parasiticide, a weak solution of corrosive sublimate. What the result will be it is impossible as yet to say, but the probability is that, though the parasite be destroyed, the nail will require a very considerable time to recover itself, for the bed of the nail is nearly as much deformed as the nail itself, being raised in the centre and depressed at the sides; many of its papillæ also are immensely elongated, so that in scraping away the nail several bleeding points were seen surrounded on all sides by a considerable depth of nail substance.

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ART. XVIII.—*On Diseases of the Supra-Renal Capsules or Morbus Addisonii.* By JOHN HUGHES, M.D.; Physician to the Mater Misericordiæ Hospital. Read at a Meeting of the Medical Society of the College of Physicians, March, 1865.

THE disease known as supra-renal melasma is a discovery of recent origin, and one which we owe to the late Dr. Addison. He it was who first traced the connexion between disease of the supra-renal bodies and a most remarkable series of phenomena which existed during life; and whatever scepticism may have existed at one time respecting his views, it would be absurd any longer to doubt their correctness; day after day cases have been recorded by competent observers, and facts have accumulated in support of his original idea, so that there is no problem in medicine better supported or more clearly proved than that which he propounded. Dr. Addison says:—

“The leading characteristic features of the morbid state to which I would direct attention are—anemia, general languor and debility, remarkable feebleness of the heart’s action, irritability of the stomach, and a peculiar change of colour in the skin, occurring in connexion with a diseased condition of the supra-renal capsules.

“As has been observed, in other forms of anemic disease, this singular disorder usually commences in such a manner that the individual has considerable difficulty in assigning the number of weeks or even months which have elapsed since he first experienced indications of failing health and strength. . . . The patient, in most of the cases I have seen, has been observed gradually to

fall off in general health. He becomes languid and weak, indisposed to either mental or bodily exertion; the appetite is impaired or entirely lost; the whites of the eyes become pearly; the pulse small and feeble, or perhaps somewhat large, but excessively soft and compressible; the body wastes without, however, presenting the dry and shrivelled skin and extreme emaciation usually attendant on protracted malignant disease. Slight pain or uneasiness is from time to time referred to the region of the stomach; and there is occasionally actual vomiting; and it is by no means uncommon for the patient to manifest indications of disturbed cerebral circulation.

“Notwithstanding these unequivocal signs of feeble circulation, anemia, and general prostration, neither the most diligent inquiry nor the most careful physical examination tends to throw the slightest gleam of light upon the precise nature of the patient’s malady; nor do we succeed in fixing upon any special lesion as the cause of this gradual and extraordinary constitutional change. We may indeed suspect some malignant or strumous disease. We may be led to inquire into the condition of the so-called blood-making organs, but we discover no proof of organic change anywhere.

“No enlargement of spleen, thyroid, thymus, or lymphatic glands; no evidence of renal disease, of purpura, of previous exhausting disease, or ague, or any long-continued exposure to miasmatic influences. But, with a more or less manifestation of the symptoms already enunciated, we discover a most remarkable and, as far as I know, characteristic discolouration taking place in the skin, sufficiently marked, indeed, as generally to have attracted the attention of the patient himself, or of the patient’s friends.

“This discolouration pervades the whole surface of the body; but is commonly most strongly manifested on the face, neck, superior extremities, penis, and scrotum, and in the flexures of the axillæ, and around the navel.

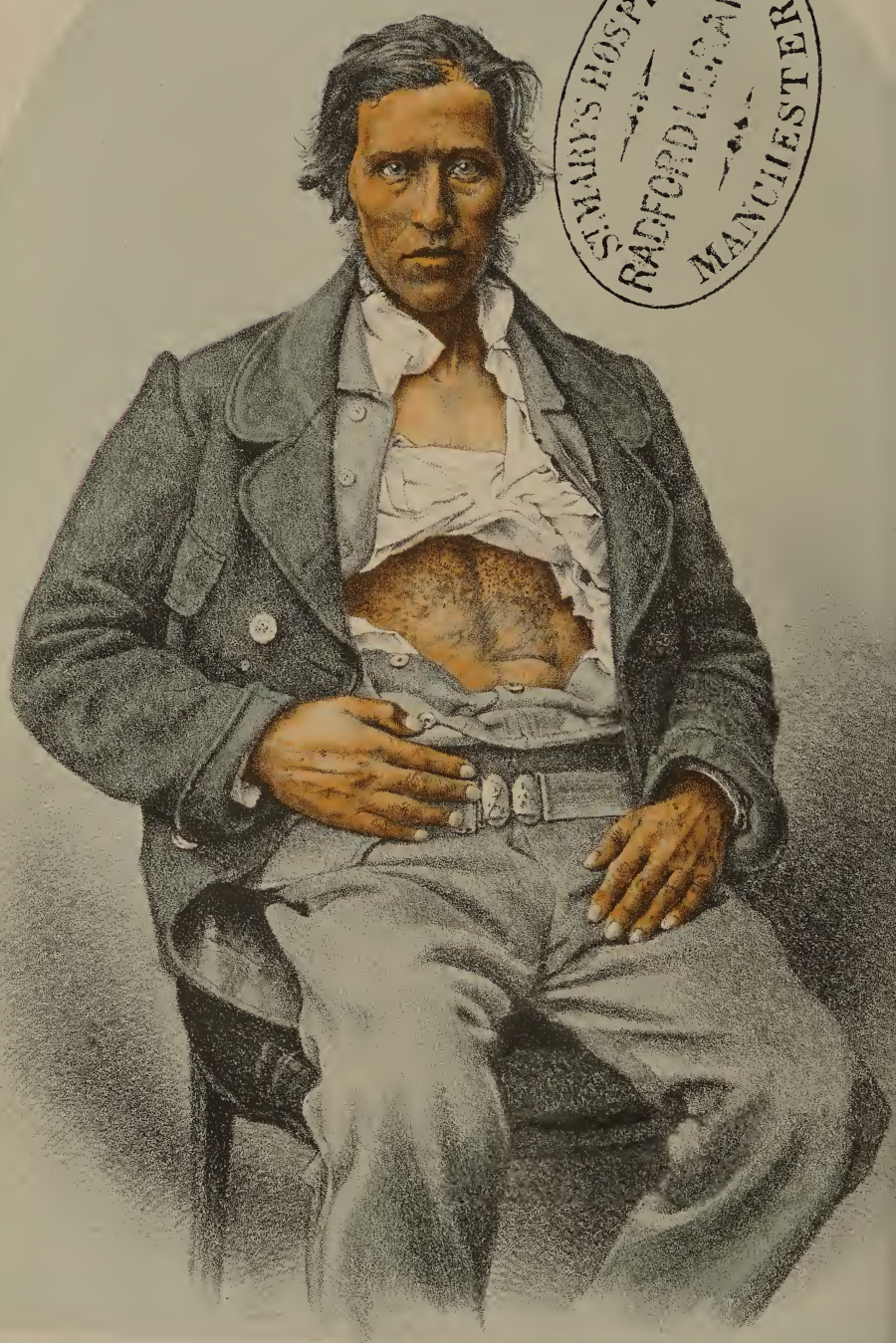
“It may be said to present a dingy or smoky appearance, or various tints or shades of deep amber or chestnut brown; and, in one instance, the skin was so universally and so deeply darkened that, but for the features, the patient might have been taken for a Mulatto.”

This description, so clear and graphic, admirably depicts the appearance of the patient and the symptoms of the disease; and, in all the cases observed since it was written, there is a singular uniformity and confirmation of its accuracy.





SIMMONS'S HOSPITAL  
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MANCHESTER.



Drawn on Stone by Henry W. Mc Connell, from a Photograph by Werner, 15, Leinster St Dublin.

MOBBUS ADDISONII DE HUNTER

Dr. Wilkes, indeed, who has written an admirable essay on this disease, and who was in the habit of examining all of Addison's cases, fully endorses his original opinions.<sup>a</sup>

He says, that Addison especially founded his diagnosis upon the constitutional symptoms as already detailed, coupled with the discolouration of the skin. This latter appearance, however, is not a necessary accompaniment of the disease, and probably depends upon its chronicity. Where it does occur, however, the whole surface of the body is uniformly implicated, and those parts most exposed are first and most affected—as the face and hands; also those parts where a blister or an eruption may have excoriated the surface.

There are other symptoms given in detail which it would occupy much space to set down here, but for which I would refer the reader to Dr. Wilkes' complete essay. I will only add—that the history, symptoms, and *post mortem* appearances of the case I am enabled to record singularly confirm the views of Dr. Addison, and closely resemble the features of some of his best marked cases; one of its most striking phenomena also had the advantage of being perpetuated by means of the photograph, and the assistance of a first rate artist who coloured it from life. The only instance of the kind, I believe, on record.

CASE.—I admitted into the Mater Misericordiæ Hospital on the 20th of February 1865, Pat. Garry, a native and resident of the county Kildare. He was a married man, had three children, was aged forty-five, by occupation a herdsman. I first saw him at my own house where he was brought by his master, who took the kindest interest in him. I asked him what ailed him, and he said he could not tell what came over him, only that he felt the greatest weakness; could not use any food, and had pains in his arms, in his back, and extending round his waist; his knees also were stiff and weak, so that when he knelt he could scarcely rise again—yet he was thankful there was no sickness upon him, only he was weak as a child, and not able to do anything. Together with those symptoms he presented the most remarkable discolouration of the skin, so peculiar, so striking, that I had him photographed on the instant, and so identical with that described by Addison that I, without hesitation, ventured on the diagnosis of supra-renal disease,

<sup>a</sup> Guy's Hospital Reports, Vol. viii.

and wrote to that effect to his master, stating also that I expected an unfavorable result. I received from him this reply:—

“MAYNOOTH, *March 3rd*, '65.

“Since the receipt of your very kind letter of the 25th I have been absent; this will, I hope, sufficiently explain my silence. Allow me to thank you for the great care and attention you showed towards poor Garry. *From your letter I anticipated the worst consequences.*”

I state this merely to show how similar in symptoms and appearance those cases are, and how easily they are recognized. The most striking phenomenon at first is the colour of the skin; but the muscular debility, the breathlessness on exertion, and the loss of appetite, all without any tangible cause, are no less remarkable. Being greatly interested in the case I used every means in my power to obtain the best history of it I could from himself, his wife, his master, and his doctor in the country. It appeared that he never suffered from any disease or accident which confined him to his bed or to the house, but for the last eight or ten years he complained of a bad stomach: its contents were frequently unduly acid, and he had occasional pyrosis, but he never vomited any blood or coloured fluid, and when the attack was over he felt no inconvenience. He was always temperate and regular in his conduct; he lived very comfortably for one in his station of life; was well clothed, well fed, and his diet was sufficiently varied, being accustomed to use fresh animal food two or three times in each week, and his occupation did not expose him to any influences detrimental to health; in fact, the most searching inquiries failed to find any sufficient cause for the gastric disturbance. His family history afforded no evidence of any hereditary taint; his father died of fever, under the age of thirty; his mother he believed to be still living in America, as also three brothers, and he has one sister at home. He had never heard of any delicacy in his family. However, the condition of his stomach troubled him for a long period; and as time wore on the attacks became more severe and more frequent. This was especially the case within the last two or three years, and for the four or five months preceding last November he could scarcely retain any food on his stomach, no matter what kind it was, and he lost condition being *one stone* short of his ordinary weight, contemporaneously with this aggravation of his gastric disturbances commencing about



last May. *The colour of his skin began to change*; every one remarked it. His friends told him he was getting jaundice; and his master told me he too observed it, but attributed the change to open air exposure. The patient himself did not mind the alteration of his colour; he only felt a gradually increasing weakness and loss of appetite. He consulted a physician of high standing in his own district, to whom I wrote, and he very kindly answered me, saying, "he was sorry he could give me little information about Pat. Garry, as he had not taken notes of his case, but he recollected that he consulted him in October last, and that he looked upon his case as one of incipient cancer of the stomach combined with disease of the liver." In December last he came to Dublin for advice, and got some powders to take and a croton oil liniment to be applied to the epigastric region. After the eruption produced by its application appeared, the vomiting ceased, but his appetite also completely ceased with it, and so also did the weakness increase. This is all I could make out of his history or antecedents bearing upon the case. On admission to the hospital he was carefully and repeatedly examined with a view of discovering some sign or symptom of organic disease to account for his extreme debility and prostration, but in vain. The lungs yielded no morbid sound either on percussion or auscultation; the heart's action was regular, and its sounds normal, but feeble, and the pulsation not to be felt in the precordial region; the pulse, too, was excessively weak and compressible, reckoning eighty in the minute. Below the diaphragm the abdomen was rather retracted; there was slight epigastric tenderness on pressure, but no tumour to be felt; the pulsation of the abdominal aorta could be readily felt in the umbilical region, and he himself complained of its throbbing, more especially when in the recumbent position, and said it kept him awake at night; the kidneys acted well; the urine was normal in quantity and quality, it contained no albumen or other deposit, its specific gravity was over 1026, and gave an acid reaction; the bowels were regular, and the tongue clean; he complained of a pain in the abdomen, and described it as extending from the lumbar vertebræ and encircling the abdomen, very severe, but intermittent; it was neither aggravated nor relieved by food, nor was it increased or mitigated by anything he knew of; he suffered also from pains in the arms, and both in the spine as well as the arms pressure was disagreeable, if not painful. There were no cerebral symptoms, no headache, or delirium, and no paralysis, but there was a remarkable depression of spirits, coupled with a nervous anxiety about himself.



In fact, our examination was negative in its results, for there was nothing established by it to account for the great weakness which existed; this latter symptom was so extreme that our patient could not walk across an ordinary room without the greatest breathlessness and distress, and he sometimes felt giddy on suddenly getting out of bed or assuming the erect position. The discolouration of the skin, as I have already observed, was very peculiar, and led me at first to make a diagnosis. My colleague, Dr. Hayden, also recognized the resemblance between this patient's symptoms and those which presented in a case he has detailed in his essay on "Supra Renal Melasma," published in number LXXVII. of the *Quarterly Journal*, and the pathological changes in which he exhibited at a meeting of the Pathological Society on the 10th of December last. My patient's complexion had been always fair and clear, and the hair brown; now the face, neck, and hands were of a smoky-brown or bronze colour, while the sclerotic was pearly white and the finger nails were also whitish, but the pigmentation was not limited to those parts; it was well marked in the axillæ; around the nipple; in the epigastric region where the croton oil liniment was applied; in the inguinal regions, especially the left, where the patient himself said it commenced—on the scrotum and over the knees; there were also in parts of the axillæ and on the arms a few scattered spots like those of purpura, but much darker, almost black.<sup>a</sup> The skin, together with being universally discoloured, was dry (the patient said he never did perspire), but not shrivelled; there was a well-marked livid bluish line along the free margin of the gums, very like that produced by the poison of lead.

Dr. Hayden kindly assisted me in making a microscopic examination of the blood; and although it contained white corpuscles, yet they were not in any remarkable excess—from five to six in each field, while in other recorded cases they amounted to forty or fifty.

From Monday, the day of his admission into hospital, till Friday there was no marked change in my patient's symptoms; indeed, he sometimes said he felt better; he got up every day, dressed himself,

<sup>a</sup> The accompanying portrait is a most faithful likeness of my patient, and gives a perfect representation of the pigmentation of the skin. The photograph was made and painted from life by Mons. Werner, 15, Leinster-street, and lithographed with singular fidelity and skill by Mr. H. M'Connell. Our patient, as may be seen from the likeness, was not a little puzzled at his novel position; and, being somewhat reluctant to submit to the photographic process, we were compelled to have him taken without much exposure of the surface; but some of the parts best marked are uncovered.

and took more nourishment than when at home. His wife came from the country to visit him on Thursday, and she thought him improved, and found him more cheerful than usual. He took six ounces of wine daily and some aromatic iron mixture. On Friday night, however, after getting into bed he had a rigor which lasted about half-an-hour, and was followed by what he called a most distressing heat (but no perspiration), and he continued so till two o'clock in the morning, when he fell asleep. At my visit on Saturday morning he told me what occurred to him, and said he had similar attacks of late, but 'this was the first since he came to hospital; he said also he took some bread and tea for his breakfast *in bed*, which he immediately threw up, and this was the first time he vomited since he came under my care, or for some weeks before. He got up, however, but was scarcely dressed when the rigor again seized him, and he was obliged to lie down. The hour of my visit on Saturday was eleven o'clock, when I carefully examined him; I found no change except some degree of febrile reaction after the rigor; his skin was hot; his pulse quick, 110, and he was thirsty; his bowels were moved early, and he passed urine; there was no pain, no tenderness, no sign of any thoracic or abdominal inflammation, and his intellect was perfectly clear; there was nothing to lead to the belief that there was immediate danger. However he began to sink that evening, and during the entire night, and at six o'clock on Sunday morning I was sent for to see him. I went to the hospital without delay, and found him lying in bed, on his back, unconscious, not able to swallow, nor capable of being roused from what might be called a quiet sleep; I could barely feel the pulse; the breathing was regular; there was no stertor, no coma, no convulsion, and his features were unchanged. He died at two o'clock.

As my patient's sojourn in the hospital was brief, and his disposition so fearful and nervous, few unconnected with the institution saw him, and those few medical friends who visited him with me were struck with the pigmentation of the skin.

The *post mortem* examination, which was made by the pupils of the hospital, in my presence, and in presence of my colleague, Dr. Hayden, twenty-four hours after death, completed the case. The morbid specimens were produced at the last meeting but one of the Pathological Society, and I will briefly repeat the observations I then made in exhibiting them:—The body was rigid, somewhat reduced, but not emaciated; the colour still remained. On laying open the the thoracic and abdominal cavities there was a deposit of yellow fat in

the latter at least half-an-inch thick; the viscera I now produce are perfectly normal; the stomach which had been so irritable and so weak of function during life, is pale, no doubt, and anemic, but without any trace of organic disease or inflammatory action; the spleen is normal in size and structure; the kidneys are perfectly healthy; the heart, which beat so feebly, is healthy in structure and its valvular apparatus perfect, nothing to remark except a more than ordinary deposition of fat upon its external surface; the lungs too, I had almost said, are healthy, for those scattered milliary tubercles on the surface just beneath the pleural membrane (but not in the substance of the lung), whatever might be their effect in process of time could not have seriously interfered with their function up to the present, neither did they reveal their existence by physical signs nor by constitutional disturbance. In vain do we search for pathological changes in the principal organs; but in examining the supra-renal bodies, here alone do we find structural alterations in the greatest degree, increase of size, and deposition of a firm opaque lardaceous-looking substance, of a yellowish white colour, something like the section of a cut parsnip. The *left* capsule is much larger than the right, as if it was in a more advanced stage of disease, but both affected by the same deposit differing only in amount; through both very large nerves run, much larger than in the normal state. Dr. Hayden has kindly obliged me by making a careful examination of this deposit under the microscope, and also of a portion of the skin which had been removed for the purpose; the following is his memorandum:—

“The cheesy-looking substance occupying the centre of the left supra-renal capsule, and deposited in the form of nodules through the right, is found, on microscopic examination, to consist of imperfect cells of different sizes, nuclei and fat molecules. There is no trace whatever of fibrous tissue, fibro-plastic material, or indeed of inflammatory products in any form—the substance is manifestly a strumous deposit. A section of the coloured skin made perpendicular to the surface with a double-edged knife, exhibits an abundant deposit of dark pigment, in the usual situation, viz., in the deepest layers of the cuticle.”

The head was not examined; but as we had no cerebral symptoms during life it is not likely we should find any material structural changes after death.

It is remarkable how closely the case recorded corresponds in all essential features with those detailed by Addison in his original essay.

The anemia, the debility, the feebleness of the heart's action, the irritability of the stomach, and the peculiar change in the colour of the skin, were all present, and also what may be called the negative symptoms of the disease, namely, the absence of any other cause to account for the phenomena present.

As already stated, Dr. Addison, and Dr. Wilkes, too, attached most importance to the asthenic condition and its symptoms as indications of this disease, and they considered the pigmentation of the skin as a late one in the order of symptoms. But, however that may be, it is one of the most remarkable amongst them, and one which attracts most attention—whether it occurs early or late during the disease, requires a larger number of cases and more minute histories to decide.

Referring to my case, it would seem to have been noticed long after the other symptoms—indeed only a few months before the patient's death, and, as I have stated, it was uniformly spread over the entire body.

Speaking of the mode in which the discolouration affected the skin, Dr. Wilkes says, “without denying that the colour may sometimes occur in patches, we think we are correct in saying that all experience has hitherto shown that the discolouration has been uniform over the whole surface of the body. The exact hue is difficult to describe. But it may be said to resemble that of a Mulatto's skin, and, therefore, is of a brownish cast, having sometimes an olive-green tinge, and thus the term bronzing of the skin, has come into use, or it has often what our artists call a walnut-juice shade.”

On the pathology of this most interesting disease, I have little to offer. In the two cases, Dr. Hayden's and my own, which I had the opportunity of examining, the deposit in the capsules appeared to me and others very like that which we meet with in other glands, and are in the habit of denominating “strumous;” and it will be remembered there were small granular bodies, which we are also in the habit of calling miliary tubercles, found on the surface of the lungs in my case; yet Dr. Wilkes, whom every one must admit to be the best authority on the subject, considers the deposit in the capsules to be *sui generis* neither cancer, melanosis, nor tubercle. He says it is deposited first as a translucent, softish,



homogeneous substance, subsequently it degenerates into a yellowish white matter, and afterwards softens into a so-called abscess, or dries up into a chalky mass. But, however, much pathologists may differ as to the ultimate structure of this deposit, and although physiologists have as yet failed to discover the special functions of the supra-renal bodies, we cannot resist the logic of facts, but must admit that their disorganization is accompanied by a very peculiar train of symptoms during life, and an amount of constitutional disturbance tending slowly, but surely, to a fatal result.

## PART II.

### REVIEWS AND BIBLIOGRAPHICAL NOTICES.

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*Lectures on the Diseases of the Stomach; with an Introduction on its Anatomy and Pathology.* By WILLIAM BRINTON, M.D., F.R.S.,  
Second Edition. London: Churchill. 1864. Pp. 368.

DR. BRINTON enjoys the reputation of having been for a great part of his medical life an unwearied workman in the field of gastric pathology. Most physicians possess a natural or acquired taste for some particular class of diseases—they would be specialists, if they could. Dr. Brinton's taste has always tended towards affections of the stomach. In the first edition of this work, published in 1858, he tells us that fifteen years before its publication his attention had been drawn to this class of diseases by the death of a near relative, and since that time he has given to them "whatever attention could be justifiably devoted to a special group of diseases, by a student and practitioner of physic in general." The fruits of this devotion to one important organ have appeared in the issue, from time to time, of several valuable essays and works, which have been received by the public with much favour. We may mention his essay on the stomach in the supplement to the *Cyclopædia of Anatomy*, which he reproduces in a revised form in the introductory chapter upon the anatomy and physiology of this viscus, which occupies the first forty pages of his present work. Again, in 1857, appeared his monograph on Ulcer of the Stomach, extending to 227 pages, our opinion of the value of which may be seen by the large amount of space which we devoted to it in our twenty-third volume. When the first edition of the present work appeared we also gave a rather extended notice of its contents in our number for November, 1859. Besides those more voluminous publications, the same subject has been handled by the author in numerous detached papers in the medical journals and Pathological Transactions. It is, therefore, amply borne out that he has made the subject one of close and continuous study; and in expecting many important results from it we shall be fully justified. The field through which he has ranged

in the acquirement and selection of his materials is a most ample and diversified one. In addition to his own opportunities as an hospital physician, he has scrutinized cases of diseased stomach in foreign hospitals, and morbid specimens of it in continental and home museums, besides gathering from the rich stores which the medical journals spread before him. To form an idea of his untiring industry we have only to look at the copious statistics which he has collected. Upon one disease of the stomach alone, namely, ulcer, he has collated from the periodicals 1,100 cases, which had never before been placed in juxtaposition. It may be inferred from all this what is the peculiar bent of the author's mind—that it is inclined more to doubt, than to admit, pretentious claims—more disposed to condense than to amplify—more ready to lop off than to retain individual details as encumbering the subject too much, and drawing the reader away from some important summary or conclusive deduction. We need not, therefore, expect to find in his book any graphic portraits, or brilliant word-painting, but *en revanche*, we have broad generalizations, ingenious and probable analogies, and an enormous store of trustworthy information upon every form of gastric disease.

It is to be expected that in the copious bibliography of diseases of the stomach much obscurity and perplexity should exist. Many of our treatises on the subject seem to have been written in *the way not to see a thing*, for when the busy practitioner rushes to them for guidance when he is embarrassed by “the stomach and its difficulties,” too often he is obliged to throw them up in despair. We want simple aphoristic rules for application to practical cases. But is the acknowledged difficulty to be sought for in the subject itself, or in the way in which it is treated, or in both? The latter is the more likely. Still the subject presents formidable impediments, so much so that it has become almost a matter of routine for writers upon it to begin by a caution to the reader not to expect perfection. Indeed it is amusing to see, in this class of works more particularly, a protest duly entered, that, notwithstanding the well-intentioned efforts of preceding writers to elucidate the maladies of the stomach, they continue in a state of deep occultation, upon which, however, the writer of the work of the day hopes and professes to let in some rays of light to enlighten such Stygian darkness. But, alas! his successor, too blind to see those new discoveries, repeats the stereotyped remark of their slowness of advancement. Even our author “is painfully impressed with the obscurity and uncertainty which

peculiarly attach (are attached?) to the maladies of the abdominal viscera." And yet it is from no desire to criticize in an unfriendly spirit the labours of his predecessors and collaborateurs that he follows in train. His object is not to cast any reflection upon the sedulous efforts of Budd, and Chambers, and Handfield Jones, who try to lay down definite bases of exploration and treatment; but simply to recognize the fact. Neither shall we offer any invidious comparisons between these writers and Dr. Brinton. We stretch out the hand of welcome to them all. When men of enlightened minds, such as they are, take up the same subject, it is almost impossible but that they view it in varying lights, and group even trite and well-known facts into interesting and novel combinations.

What is the cause of the difficulty which is universally allowed to beset stomach diseases? The cause is not a single one, but a plurality. We have to do with an organ possessed of the widest range of physiological and pathological combinations. As the great organ of sustentation, the stomach may be likened, to use a rather homely illustration, to a grand central junction, to which the various trains return for coaling, and, when supplied, again branch off from it towards their widely-distant and independent termini. As our author observes:—

"No matter what the organ or what the disease, it is by the stomach you will have both to assault the enemy, and to victual the beleaguered fortress of life you come to help. Indeed, the classical fable of 'The Belly and the Members' may suffice to indicate how the income and expenditure, the pleasures and pains, the health and disease of the stomach are alike vicarious and ministerial." (p. 41.)

We can never read that beautiful apologue of the revolt of the members from the belly here alluded to, and so well told by Livy, though he apologizes for its old-fashioned style ("prisco illo dicendi et horrido modo") without being struck as well by its apposite application, as by its foreshadowing the future discovery of the intermixture of the chyle with the blood. We give Livy's summing up:—

"It thence appeared that the office which the stomach had to discharge was not a slothful one, and that if it be fed itself, it also feeds the rest of the body by returning to all parts of it that blood from which we get life and strength, distributed in regular proportions



into the veins, after it has been brought into a mature condition through the digestion of the food." (*Tit. Liv. Lib. II.*, cap. 32.)

Besides its prominence as the source of all support—a fact which we should never lose sight of in the treatment of disease—a further complication arises from its being the centre of sympathy, quickly taking the alarm when some disturbance arises amongst the distant members and organs. There, too, a difficulty arises from the similarity of features presented by its sympathetic disorders to those of its own independent diseases.

But a still stronger cause of the uncertainty which still hangs about these diseases is to be found in an imperfect knowledge of gastric physiology. In some of the recent works on the stomach, to which we have alluded, it has been attempted to run a course of parallel lines between its physiology and pathology. The former, no doubt, is the key by which we interpret the mystical cipher of the latter; but we should be satisfied that the key is a true one, and not a scientific picklock or a forcible wrench. There is something so fascinating about physiology that it is no wonder that men of science are sometimes led astray by it; and practical men have then to renounce as bases for treatment those boasted discoveries, those *idola theatri*, which have been deemed for a time irrefragable. Still, notwithstanding many illusive flashes, much light of a more certain and steady ray has been let in upon its dark places, and, according to Dr. Brinton, it is even now just emerging from uncertainty. Until the famous case of Alexis St. Martin, and the well devised experiments of Dr. Beaumont appeared, the opinions entertained upon the natural history of digestion were crude and conjectural. Dr. Brinton supplies us with many illustrations both of the doubtfulness and success of physiological theories. As an example of the former we may rank the hitherto unsuccessful attempt to fix the true acid of the gastric secretion. He gives a startling array of eight analytical authorities, who pronounce that it is hydrochloric acid; and by a sort of chemical neutralization he cites eight others, equally illustrious, who decide that it is the lactic. We have somewhat to say against him for passing over the name of Graves amongst this latter assemblage, whose claim has, we believe, not as yet been denied of having been the first to propose, in 1823, the lactic acid as the acid of the gastric fluid.

On the other hand, as an example of successful advance in physiological discovery, Dr. Brinton adduces the settlement of the

famous question of the self-digestion of the stomach, which, since John Hunter's time, has been, what it may be a Hibernicism to call a *bone* of contention amongst physiologists. He regards the experiments of Dr. Pavy as decisive. It is undoubtedly a singular paradox, why it is that the stomach, by a suicidal act, is not *always* digested by its own juices, when these same juices are capable of dissolving flesh as food? And then comes a second question, equally singular, and a corollary of the former, viz., why it is *occasionally* a solvent of itself? Cases of the latter occurrence, wherein the suspension of the general law of immunity takes place, have been noticed by various writers, and, of course, numerous theories have been hazarded to account for it. The first point was to collect, by a large induction, examples of the various cases in which it occurred—the *tabulæ instantiarum* of Bacon. Thus it has been found to exist after violent deaths of persons in rude health, especially shortly after a full meal; and this has been naturally attributed to the out-pouring of the gastric secretion at the time. Again, it has been found to occur during several diseases, one of the chief being phthisis, and Cruveilhier and Rokitsanski both cite forms of stomach solution which they suppose to be partly due to this cause. Dr. Budd argues in his work that it is wholly attributable to the gastric juice being secreted at other times than during chymification. It was in this state that Dr. Pavy, by his experiments, according to Dr. Brinton, closed the controversy about the first question:—

“He has shown that it is neither to any abstract vitality of the living stomach, nor even to the continual dilution (and therefore neutralization) of its interstitial liquids, by the salivary or other contents of the organ occupying its surface, that this immunity is to be referred; but to a protection afforded the stomach by the constant irrigation of the organ with circulating blood. His well-selected experiments prove that the mass of the organ is thus, as it were, perpetually neutralized by the alkaline blood-stream, and thus kept free from the process of solution for which a free acid is a necessary condition; and that while an arrest of the circulation permits the stomach to dissolve its own walls, this solvent effect is aided by artificial acidulation, and impeded by alkalizing, much in the degree which on such a theory might be predicated.” (p. 38.)

Dr. Brinton has, in his third lecture, applied this theory to explain how the gastric juice *begins* any destruction or solution of the stomach:—

"Dr. Pavy's observations, while they remit to further researches the task of deciding how far an undue acidity of gastric juice may inaugurate or enlarge an ulcer, have the broad substantive merit of indicating that any such extreme congestion as can retard, and (however temporarily) arrest, the flow of blood through a given area of gastric mucous membrane, may thus allow this energetic secretion to cause, what with more or less of adjacent reaction, constitutes an ulcer of the stomach" (p. 164).

This "invaluable piece of information" affords the desired clue to the formation of gastric ulcers:—

"Stagnation of gastric blood . . . seems thus to be a main condition, a *causa causativa*, of gastric ulceration, which, in so far as it is the effect of a chemical act of solution brought about by healthy gastric secretion, constitutes a process very unlike that which we elsewhere connote by this term. The ulcer, in short, seems to be in its outset a lesion of the vaso-motor system—an accident, so to speak, of extreme gastric congestion."—(*Ib.*)

Besides our imperfect knowledge of the physiology of the stomach the community of symptoms in most of its diseases presents another ground for our want of precision in their detection. It has been often observed that few, if any, of the affections of this viscus possess an exclusive right and title to any one symptom, and it is well known that symptoms, apparently of the greatest severity and most dangerous import, may, and do, exist, without anything more than functional disturbance; while, on the contrary, organic affections, even compromising life, may not be revealed by any prominently-marked symptoms.

The last source of difficulty on which we shall dwell is one of the most important, but for which future discovery may yet do much, namely, the want of special aids of diagnosis:—

"The aids to diagnosis afforded by auscultation in the diseases of the thoracic viscera, and by chemistry in those of the urinary apparatus, scarcely find any parallel in the maladies of an organ which executes its work without perceptible sound or movement, and only dismisses its products from the body after a complex series of changes and admixtures." (p. 42.)

It is well to know on what we are to depend for the diagnosis of stomach diseases. The author gives a brief summary of such aids

as do exist; we are sorry that they are not more numerous. The foremost place is assigned by him to physiology:—"We shall look to physiology to explain their symptoms, to aid their progress, and to dictate their treatment." He also dwells upon the value of those physical means of examining the abdomen, analogous to percussion and auscultation in thoracic disease; among these we may reckon inspection, palpation, measurement, and percussion of the abdominal walls, and as one of the first to give to these means of information their due value, we must admit that medical science is much indebted to M. Piorry, of Paris. The author further advises the collection and comparison of the histories of large numbers of cases by which we may learn the symptoms which are most essential and constant, a mode of inquiry in which he himself has been most successful. He accounts for the extraordinary powers of diagnosis possessed by some physicians by their minute study of symptoms in contrast with the lesions found after death. This "will sometimes enable us to refine our own diagnosis to a degree which borders on temerity." Of course this must depend, in the case of individual observers, upon a combination of various causes, such as acute organs of sense, associated with the important mental faculties of compounding, abstracting, and comparing. Invaluable information may likewise be obtained from the chemical and microscopical examination of the substances expelled from the digestive canal, guarding, of course, against the many sources of error connected with them. He does not even disdain to take a hint from the occasional successes of modern quackery, while, strange to say, he omits altogether the experimental deductions which orthodox remedial agents afford, according to the maxim of Hippocrates, "*Naturam morborum ostendit curatio*," unless, indeed he facetiously intended to include the latter in the former category. It must be allowed that no single one of all these possesses much intrinsic weight, still we are far from depreciating their collective value.

In our review of the first edition of this work we gave a rapid analysis of the first six chapters, and it would be merely *actum agere* to recur to the subject. Our attention shall, therefore, be directed to the seventh and eighth lectures, which now appear for the first time. The first of these is upon the subject of gastric phthisis; the other is headed "*Gout in the Stomach.*" They are written with different aims; the former is intended to inaugurate the existence of a special kind of gastric disease, which heretofore has escaped notice, being supposed to be one of the Protean forms of pulmonary



phthisis, rather than a disease *per se*. The lecture upon gout in the stomach has the contrary aim of extinguishing its pretensions to the very popular position which it holds in the published treatises upon that disease. According to Dr. Brinton, gout in the stomach is a myth, a medical bugbear, calculated to terrify impenitent *bon vivants* and timorous senators, but possessing a traditional rather than a real origin and descent. The lecture on gastric phthisis occupies only fourteen pages of the book, and gout in the stomach, is dismissed in eight, both together occupying not one-third of the space assigned to cancer, or one fourth of that given to ulcer of the stomach. Perhaps this disproportion arises from their being new topics upon which the voice of medical science has not yet pronounced. In considering these novel claims we naturally inquire what the author means by the expression gastric phthisis—and we feel rather at a loss to form a complete conception of it. Even the author himself seems to feel a degree of doubt in his mind, a kind of *arrière pensée*, that it is not quite so clear as it might be; and in our humble judgment his account of it lacks some of the distinct and incisive characters which distinguish other parts of his book. Thus he seeks to explain its nature, not so much by direct description as by its analogies:—

“Without attempting to define, scarcely even hoping to explain its exact nature, I venture to point out those analogies which seem to me to indicate the true position of the gastric malady, and especially its relation to ordinary tubercle of the lungs.” (p. 347.)

Before examining these analogies, we must, however, follow the author, who, *plume a la main*, sketches at the bedside the prominent features of a typical case, as follows:—

It begins as an indigestion, the first symptom of which is pain, commencing one or two hours after eating, and at first consecutive upon the morning repast, but afterwards following all the meals. The next symptom seems to be nausea, which arises from the sickening sensation left by the pain; and this passes through its degrees till it amounts to retching, but seldom goes so far as vomiting of the food. It seems at length to proceed to such a pitch of gastric irritability that pain and retching succeed almost immediately the act of taking food, and even prevent that act by the unbearable suffering accompanying deglutition. It is of great importance for the diagnosis of the case to trace the phthisical history

of the patient's relatives. This, however, is not so applicable to cases of gastric phthisis as to those of ordinary tubercle of the lungs. By taking a group of relatives of a phthisical family we may find two or one attacked with the gastric form, "while another succumbs to the characteristic intestinal form of tuberculosis." The symptom next in diagnostic value is the state of the lungs, and

"Paradoxical as it may sound to say so, it is quite as much by the absence of some thoracic signs as by the presence of others that the results of careful auscultation indicate a given case to belong to the category of gastric phthisis." (page 344.)

He seldom has met it in concurrence with even moderate aggregations of tubercle in the lungs, still less with the more advanced stage of tubercular disintegration. Fever, as a concomitant symptom, is a valuable guide, especially if taken in connexion with the absence of thoracic lesion; it is always of higher intensity than in ulcer of the stomach, or any form of dyspepsia. Laryngeal phthisis forms its nearest resemblance; if we subduct the symptoms connected with the cough, voice, and breathing, there is a great parity of symptoms in both. The absence of hemorrhage affords a safe distinction between gastric phthisis and cancer or ulcer of the stomach. In his experience the stomach is more bloodless than congested.

In seeking for the causes of this *nexus* of symptoms he finds ample proof of their nervous origin. The increased sensibility, taken in connexion with the vomiting, points to a great disturbance of the motor functions, and implies a partial or general irritation of both sensitive and motor nerve-elements, while the gastric juice is reduced to a minimum.

Dr. Brinton looks diligently out for some analogy to explain the nature of this affection on the principle of Bacon's *instantiæ parallele*, and at last finds one in ordinary face-ache. He inquires into the elementary phenomena of facial neuralgia, and notes four, viz., (1) excessive pain; (2) undue sensibility; (3) increased heat, vascularity, and interstitial fluid; (4) muscular twitching and spasms; and traces at some length their correlative analogues in gastric phthisis. He finally arrives at the conclusion that this disease is a *species of neuralgia of the pneumo-gastric and sympathetic nerves*.

Dr. Brinton is very hopeful about its successful treatment, if taken in time; he suggests as the basis of treatment the remedies which appease undue irritability of the nerves of the stomach, and

the avoidance, especially, of mercury. Alkalies, and the effervescing citrates and tartrates are very useful. Iron is beneficial, after cautious preliminary preparation. This ailment yields much more readily than gastric ulcer.

We have not space to discuss his reasons for disbelieving in the entity of gout in the stomach. His mode of argument is that all the supposed cases can be sufficiently accounted for by classifying them under other heads, of which he gives four, viz.: (1) painful digestion, (2) biliary colic, (3) renal degeneration, and (4) cardiac disease.

We now part with Dr. Brinton with a feeling of deep and deserved respect. Our opinion of his philosophical work is very high. It is a repertory of most valuable information upon diseases of the stomach, worked out in no small degree by his own original ingenuity and research, and evidently the result of much and deep thought. Any one chapter of it would form a subject for a lengthened review. His book is a valuable accession to our medical literature.

1. *Researches on the Intimate Structure of the Brain, Human and Comparative. First Series:—On the Structure of the Medulla Oblongata.* By J. L. CLARKE, Esq., F.R.S.—(*Philosophical Transactions*, Part I., 1858.)
2. *Further Researches on the Grey Substance of the Spinal Cord.* By J. L. CLARKE, Esq., F.R.S.—(*Philosophical Transactions*, Part I., 1859.)
3. *Microscopic Anatomy of the Lumbar Enlargement of the Spinal Cord.* By JOHN DEAN, M.D. Cambridge. 1861.
4. *The Grey Substance of the Medulla Oblongata and Trapezium.* By JOHN DEAN, M.D. Washington. 1864.
5. *Researches on the Development of the Spinal Cord in Man, Mammalia, and Birds.* By J. LOCKHART CLARKE, F.R.S.—(*Philosophical Transactions*, Part II. 1862.)

ALTHOUGH our knowledge of the minute anatomy of the central nervous system is of comparatively recent date, and although much

of it is still uncertain, yet no one who reads with attention the works whose titles head this article can deny that much has already been done, and that, at least, a sure and solid foundation has been laid on which future investigators may build with security in their endeavours to elucidate those points on which we are still in ignorance or doubt.

The earlier papers of Mr. Lockhart Clarke have already been noticed in recent numbers of this Journal, and many of his results have found their way into our text books of anatomy and physiology; in the present review we shall, therefore, devote ourselves principally to the analysis of the works of Dr. Dean, merely referring from time to time to the investigations of Clarke, whose conclusions are, in every important particular, confirmed by those of the American physiologist.

We shall first consider his work on the grey substance of the lumbar enlargement of the cord.

Although cells are to be found abundantly in every part of the cord, yet *nerve* cells are confined to the grey substance, and their presence constitutes the great histological distinction between this and the white or fibrous substance in which any cells that exist belong to the connective tissue.

In the grey substance are found nerve cells, connective tissue, and nerve fibres running in various directions. A nerve cell appears to be constituted by an enlargement of the axis cylinder, containing a granular substance and a nucleus; the existence of a cell wall is still doubtful. The cells vary much in size, shape, and number of processes, but the distinctions which Jacobowitsch has attempted to draw between motor, sensitive, and sympathetic cells, founded on these variations, is quite untenable. The author has little to add to the account given by Clarke and Stilling of the cells in the anterior cornua, but with regard to the arrangement of cells in the posterior cornua some uncertainty still prevails; the opinion of Bidder and Kupffer that all the cells in the posterior horns belong to the connective tissue is manifestly erroneous, as Clarke and the author have found nerve cells abundantly, and given drawings of them. The principal cell groups are found (1) in the fibrous band which borders the substantia gelatinosa, where their processes are connected with the posterior nerve roots; (2) at the junction of the cervix and caput cornu, where they extend out into the white substance, with the fibres of which their processes form an intricate interlacement; and (3) in the cervix, where they



represent the posterior vesicular columns, which have been wrongly supposed to be absent in the lumbar region.

That nerve cells are frequently connected with each other by their processes, can admit of no doubt; but on this subject the author urges caution, for a year's observation has convinced him that the undoubted examples of cell connexion *seen*, even in the most favourable specimens, are *exceptional* rather than constant. Cell connexion is more frequently seen in the anterior than in the posterior horns.

With regard to the connexion between the nerve-cells and the roots of the spinal nerves considerable difference of opinion exists. After giving the views of several writers on this subject the author concludes that, as regards origin, there are the three following classes of nerve-roots:—

“I.—(a) Anterior roots, which arise from or terminate in anterior cells; (b) posterior roots, which arise from or terminate in posterior cells.

“II.—Anterior and posterior roots which meet in cells in the central part of the cord.

“III. Anterior and posterior roots which are directly continuous, *i.e.*, unconnected with any cells in the cord.

“I am, however, very far from pretending to imply any supposed difference of function between these classes, for I am very strongly convinced that the *function* of cell and fibre is everywhere the same; and one of the principal objects I had in view in the above classification has been to show how closely anterior and posterior roots are connected, and how nearly they come to having a common origin.” (p. 10.)

The author gives a drawing of a cell from the posterior vesicular column, which is connected by its processes with four distinct bundles belonging to the posterior roots:—

“It is highly probable that these four bundles proceed from different, if not distinct, parts of the body, so that *possibly we have here sensations from four more or less distinct parts of the body centralized in one nerve-cell*. How they are separately conveyed to the sensorium as distinct sensations I have been able to form no idea.” (p. 9.)

With regard to the connexion of the cells with the longitudinal white fibres, the same difference of opinion exists as on the subject of the connexion between the cells and nerve-roots. Bidder and Schroeder Van der Kolk believing that *all* longitudinal fibres of the anterior and lateral columns arise from cells, while Stilling asserts

“that the whole white substance of the spinal cord (like the separate columns) has a double origin—peripheral and central.”

The author, while believing that no part of the anatomy of the cord requires more careful investigation than the constitution and origin of the white columns, adopts the following views provisionally:—“The anterior and lateral columns, apart from the anterior roots, are only *partially* derived from the cells of the anterior and posterior cornua, *some* of the white longitudinal fibres seeming to be direct continuations of the posterior roots, after these have passed through the grey substance. The posterior white columns are composed almost exclusively of the posterior roots, a few fibres appearing to be derived from cell processes coming from the large cells, situated on the margin of the posterior cornu; what course these latter fibres take after leaving the grey substance I have been unable to determine definitely.” (p. 10.)

*Course of the nerve-roots.*—The posterior nerve-roots are attached solely to the posterior columns, having no direct connexion with the lateral columns, as maintained by Stilling and others.

In the posterior columns the fibres appear to take four principal courses. 1. Some ascend obliquely inwards, entering the grey substance sooner or later, according to their degree of obliquity. Though it is probable that all these fibres enter the grey substance ultimately, it is not yet anatomically proved, for many are so nearly longitudinal that they cannot be followed in their entire course. 2. Some run obliquely at starting, but soon assume a directly transverse course, and in this direction pass deeply into the grey substance. They are less numerous and finer than the fibres of the first set. 3. Some descend obliquely inwards, pursuing in a downward course a direction the exact counterpart of that pursued by the first set in ascending. Though some of the latter may be supposed to reach the brain without entering the grey substance of the cord, there can be no doubt that all the descending fibres enter the vesicular columns, but the same difficulty in tracing the whole course of a fibre exists in each case. 4. Some are looped or recurrent, uniting ascending and descending fibres:—“Their course is usually as follows:—Starting from the ascending end of the bundle, the fibres penetrate the posterior column as far as the margin of the grey substance; here the bundle often divides, part of its fibres passing upwards, whilst a part penetrate the grey substance, pursuing a slightly oblique ascending course. They frequently proceed upwards for a considerable distance, finally looping round

and re-entering the white column, joining some bundle, with which they pass out, either the root immediately above their entrance, or, as is often the case, one higher up." (p. 13.)

*Central course of the nerve-roots.*—In longitudinal sections, the ascending, descending, and transverse fibres are seen to pass indiscriminately into transverse bundles, which traverse the substantia gelatinosa, and then curving upwards or downwards form those longitudinal bundles which are seen near the junction of the cervix and caput cornu; these the author particularly describes under the name of the "longitudinal columns of the cornu." After a short longitudinal course the fibres forming these bundles pass forwards, some to the commissures, some to the cells in the anterior cornu; some to be directly continuous with the anterior nerve-roots, and some into the antero-lateral columns of the cord. Some of the transverse bundles do not enter into the longitudinal columns of the cornu, but pass directly forwards, and are either lost to view or are seen to be directly continuous with the anterior nerve-roots. In the *anterior white columns* are found:—1. The fibres of the anterior nerve-roots, which, after pursuing an obliquely ascending course enter the grey substance. 2. Fibres forming loops; these pass out of the grey matter often in company with an anterior nerve-root; they then curve downwards, and after a variable distance again enter the grey substance. While agreeing with Clarke that some of these loops may be derived from the posterior nerve-roots, Dean believes that the greater number of them have an origin from the cells of the anterior cornu, with which they are often seen to be connected at both extremities.

In conclusion, the author sums up the chief results arrived at in a number of propositions.

The paper is illustrated by four well executed plates, containing eight figures.

We have next to consider the memoir of Dean on the grey substance of the medulla oblongata. This forms one of the publications of the Smithsonian Institution—it is illustrated by sixteen plates, containing forty-five figures, many of which are lithographic reproductions of photographs taken by the author from sections of the medulla; they are exceedingly accurate and well executed, and the whole work bears evidence of much patience and labour.

As the grey matter passes from the cord into the medulla oblongata the posterior cornua are pushed forwards and outwards, and their outer part is broken up into a net-work by the passage

through it of numerous longitudinal and arciform fibres. The caput cornu, thus pushed forwards, approaches the surface of the antero-lateral column forming the tuberculo-cinereo of Rolando, while from the posterior aspect of the cervix cornu are developed two tufts of grey matter, which have been described by Clarke under the names of post-pyramidal and restiform nuclei. The tractus intermedio-lateralis is still apparent, but gradually diminishes as the nucleus of the spinal accessory is developed. The anterior cornua are broken up into a net-work by longitudinal fasciculi of fibres, and are traversed by the roots of the hypoglossal nerve. Above the crossing of the pyramids the raphè becomes apparent; it is the seat of a most complex decussation between fibres from nearly all parts of the *opposite* sides of the medulla, chiefly, as shown by Clarke, through the system of arciform fibres, which at the same time connect together all the parts of each *separate* half. —(Medulla Oblongata, p. 251.) As we ascend the olivary bodies appear; these are very closely connected with each other and with the nuclei of the different nerves, specially with that of the hypoglossal. This large nucleus is developed on the antero-lateral aspect of the central canal, while posterior to the canal is found the nucleus of the spinal accessory; the connexion between these two nuclei is of the closest description, and will be again referred to. In each anterior pyramid of man we find a group of cells, the great pyramidal nucleus, believed by Clarke and Dean to be a portion of the olivary body which it much resembles in the arrangement assumed by its cells; but supposed by Stilling to be the chief source from whence the fibres of the anterior pyramidal columns arise. In the lateral column also a group of cells is found, which, under the name of “antero lateral nucleus,” has been particularly described by Dean, who considers it to be “a distinct group of cells intimately connected with the development of the internal arciform fibres.”—(p. 7.) Ascending, as we come to the level of the calamus scriptorius, we find the spinal accessory nucleus being gradually transformed into that of the vagus. At this level the olivary bodies, the post-pyramidal and antero-lateral nuclei have reached their greatest development. Higher up the antero lateral nucleus is encroached upon by the olivary body, and between the post-pyramidal nucleus and that of the vagus is developed the auditory nucleus. This enlarging pushes forward the vagus nucleus, which gradually is transformed into the nucleus of the glosso-pharyngeal. The hypoglossal nucleus now diminishes in size, and by its upward



extension forms the fasciculus teres, from which, by a common nucleus, arise the sixth nerve and the portio dura of the seventh. The olivary bodies diminish in size, the restiform nuclei persist, and from them fibres radiate obliquely, becoming more and more horizontal as the cerebellum is approached. The posterior division of the auditory nerve is seen winding round the back of the restiform bodies. Finally, the olivary and anterior pyramidal columns give place to the complex structure of the pons Varolii. Having thus given a brief sketch of the morphological changes observed as we ascend through the medulla, we proceed to notice more particularly some of the principal points in its anatomy.

*Hypoglossal nucleus and roots.*—This nucleus appears just above the upper cervical nerves, in front of the central canal, and is developed in the posterior portion of the anterior cornu, being evidently a continuation of the cell columns from which the anterior nerve roots arise; indeed, it is difficult to say where the fibres of origin of the upper cervical nerves terminate and those of the hypoglossal begin. The nucleus is pyramidal in shape, and contains large multipolar cells, arranged in groups, and whose processes, radiating in every direction, establish communications between the nuclei of opposite sides, and between each nucleus and other parts on the same side, principally the spinal accessory nucleus and the olivary body. The latter communication is very intimate, and is effected both by direct fibres, and by little cell groups scattered along the raphè and hypoglossal roots. In most mammalia the hypoglossal roots pass to their nucleus on the outer side of the olivary body, but, in man the greatly increased development of the olivary body which is one of the chief characteristics of the human medulla, necessitates the passage of the nerve along the inner side of the ganglion, between it and the anterior pyramid. Arrived at its nucleus the greater number of fibres are connected with the groups of multipolar cells, but some pass backwards to the nuclei of the spinal accessory and vagus, and some turn inwards, and, decussating at the raphè, pass to the opposite hypoglossal nucleus.

Passing over an interesting chapter on the passage into the medulla of the posterior vesicular columns and tractus intermedio lateralis, we proceed to speak of the *vagus nucleus, and roots*. The nucleus is an upward extension of the nucleus of the spinal accessory, the line of demarcation being difficult of determination. In shape it is triangular, the apex being bifid, from the passage through it of longitudinal fibres, which, passing into the cord,

connect the vagal nuclei with the whole class of respiratory muscles. The cells are mostly oval or fusiform, and vary much in size, the largest cells being in the anterior part of the nucleus, near to the entrance into it of the nerve-roots. Their processes pass in various directions, and establish numerous connexions between the vagal nucleus and other parts, principally the hypoglossal, post pyramidal, restiform, and opposite vagal nuclei; also the longitudinal fibres already spoken of, the caput cornu, and the fibres of the trifacial, which Clarke has shown to descend in the grey matter of the posterior horn.

The roots of the vagus pass inwards through the caput cornu, and through the descending fibres of the trifacial, but are not *directly* continuous with these; they for the most part enter the anterior portion of the nucleus and become continuous with its cells; some pass backwards around the longitudinal bundles, and enter the posterior portion of the nucleus; some pass forwards, and either decussate at the raphè or pass into the hypoglossal nucleus, where they join the cells, and probably in some cases are *directly* continuous with fibre of the hypoglossal roots. "If such be the truth, we have in the medulla three classes of nerve fibres analogous to those I pointed out formerly as existing in the spinal cord, viz.:—1. Vagus (spinal accessory) and hypoglossal roots which arise from or terminate in cells in their respective nuclei. 2. Vagus (spinal accessory) and hypoglossal roots meeting in cells. 3. Vagus (spinal accessory) and hypoglossal roots directly continuous."—(p. 29.)

*Glossopharyngeal nucleus and roots.*—We have already described how the auditory nucleus is developed between the post-pyramidal and vagal nuclei. As it enlarges it pushes forwards the nucleus of the vagus, which gradually passes into the glosso-pharyngeal nucleus, there being no defined separation; the three divisions of the eighth pair of nerves may, indeed, be described as coming off from the same nucleus, the glosso-pharyngeal highest up, below this the vagus, and lowest of all the spinal accessory. The roots of the glossopharyngeal pass in through the caput cornu and pierce the descending fibres of the trifacial, but are not directly connected with them. Some of the fibres enter the nucleus, and are connected with the cells, others pass to the fasciculus teres, the upward extension of the hypoglossal nucleus, others pass into the anterior and lateral columns, and either decussate at the raphè, or pass upwards as longitudinal bundles. The decussation of the roots of the

glosso-pharyngeal nerve is difficult to trace in the human subject, but in the cat it is very distinct.

With regard to the minute structure of the olivary bodies, the author confirms in every particular the account given by Clarke. This question is of much interest, not only as being one of the most difficult points in the anatomy of the medulla, but from the probability, noticed by Dean and Clarke, that the same general plan exists wherever convoluted masses of grey substance are formed, so that the study of the minute anatomy of these bodies is likely to throw much light on the analogous structure of the cerebrum and cerebellum. With regard to the function of the olivary bodies, the author agrees with Clarke in believing them to exert a co-ordinating influence on the other ganglia of the medulla, an opinion founded on the fact of their close connexion with all these ganglia, while they are immediately connected with no nerve fibres.

In mammalia the olivary bodies are less developed than in man, and more clearly divided into superior and inferior.

In the second part of his memoir the author, after having described the morphological changes which we find to take place in the trapezium of men and mammalia, as we ascend towards the pons varolii, proceeds to consider the remaining ganglia found in this part of the nervous system.

*Auditory nucleus and roots.*—We have already described the position in which the auditory nucleus is developed; as it enlarges it pushes forwards the vagal nucleus, with which, and that of the hypoglossal, it becomes fused into one mass. Higher up it is pushed backwards by the fasciculus teres. In the auditory nucleus some of the largest nerve cells in the body are to be found, whose processes pass in all directions, establishing numerous connexions between the nucleus and other parts; that with the cerebellum being particularly intimate. The auditory nerve consists of anterior and posterior roots. The posterior wind round the back of the restiform bodies, and form the well known white streaks on the floor of the fourth ventricle. They contain many ganglionic cells, and are reinforced by fibres from the restiform bodies; they pass into the auditory nucleus, and are also connected with the nucleus of the facial and abducens. Many fibres decussate at the raphè. Van der Kolk supposes that the posterior division of the auditory root is unconnected with the sense of hearing, and consists of reflex fibres which are “connected by means of ganglionic cells with



the auditory nerve, and in terror, caused by an unexpected sound, reflect upon the whole muscular system, and place the body in an attitude of defence." The anterior division of the auditory root is thus described by Clarke:—"It consists of two portions—the principal portion penetrates the medulla beneath the restiform body, and, running along the outer side of the caput cornu, enters both parts of the auditory nucleus; the other portion runs backwards, along the upper border of the restiform body, which it accompanies over the superior peduncle to the inferior vermiform process of the cerebellum." Much uncertainty still prevails with regard to the ultimate destination of many of the root fibrils of the auditory nerves.

*Facial nucleus and roots.*—"The facial nerve enters the medulla on the inner side of the caput cornu, and runs in a curving course directly inwards and forwards to the fasciculus teres, which is an upward continuation of the column of large multipolar cells with which the roots of the hypoglossal were connected lower down." A remarkable difference exists between the upper and lower fibres of the nerve; the latter proceed to the nucleus and there terminate; while the former, which are on a higher level than the nucleus, pass to the raphè, and decussate with fibres from the opposite nerve. In this course they enclose a bundle of longitudinal fibres, supposed by Stilling to belong to the fifth nerve, and by Van der Kolk to the auditory. Dean adopts neither of these opinions, but thinks that many of the facial fibres join these longitudinal bundles, and passing downwards either decussate or enter their own nucleus. Stilling has supposed that fibres of the facial pass downwards into the columns of the cord. There is no nerve in the medulla which decussates so freely as the facial; some of its fibres must pass up as high as the corpus striatum and optic thalamus, for in ordinary hemiplegia, from disease of these ganglia, the face is almost always partially paralyzed on the side opposite to the lesion.

*Abducens nucleus and root.*—While all the other nerves of the medulla pass inwards towards the raphè, where they decussate more or less freely, the abducens in its course towards its nucleus bends outwards, and consequently does not decussate, at least directly. According to Stilling and Clarke the abducens arises from the same nucleus as the facial. Van der Kolk thinks that

<sup>a</sup> Dean thinks that anatomical facts are wanting to support the theory of reflex influence exercised by the auditory on the facial nerve, which has been so ingeniously set forth by Van der Kolk. *Medulla oblongata* (New Syd. Soc.) p. 124.



after piercing the roots of the facial it passes upwards to a nucleus of its own, which is connected by fibres with the nucleus of the opposite motor oculi. Dean traces some of the fibres of the abducens into the facial nucleus, others to the back of the auditory nucleus, and along the floor of the fourth ventricle; and thinks, on theoretic grounds, that the connexion with the opposite oculo motor nucleus is not improbable.

*The upper olivary bodies* consist of a group of stellate cells developed on each side of the raphè, above the lower olivary bodies, and in the situation of the antero-lateral nuclei, of which they are the upward extension. Van der Kolk, finding them most highly developed in animals whose power of expression is greatest, concludes that they are accessory nuclei for the facial, just as, according to him, the lower olivary bodies are for the hypoglossal nerve. Dean, while admitting the close connexion between the facial nuclei and upper olivary bodies, thinks that as the most intimate relations of the latter are with the great system of arciform fibres we must look on them as performing the same function as the lower olivary bodies, namely—co-ordination of the different parts of the medulla and trapezium.

The memoir terminates by a description of the methods employed in the preparation of the medulla for microscopic examination, and for photographic use.

We have thus followed the author, step by step, through his essay; and if in any instance we have failed to make his meaning plain to our readers, the difficulty of condensing that which is already so condensed, and of explaining the complex structure of the medulla without the aid of diagrams or preparations, must be our excuse. We hope that any defects in our review will have the effect of making our readers refer to the original works, where we can promise them an ample reward for the time they will spend in their perusal. Like our author we have confined ourselves to a bare statement of facts, and have avoided drawing any conclusions from them. It would not be difficult to give a list of physiological and pathological phenomena capable of explanation by these facts, and we can bear personal testimony to the light which in practice they are able to throw on many obscure diseases of the nervous system.

The last work on our list is that of Mr. Lockhart Clarke on the development of the spinal cord. As our remarks have already extended to too great a length we forbear, on the present occasion, to give any detailed analysis of this valuable memoir, to which we

should be unable to do justice in the limited space at our disposal. We can, however, recommend it most heartily to the attention of our readers, who will find it marked by the same patience in research, accuracy in observation, and clearness in style that characterize all the writings of its author.

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1. *De l'Ataxie Locomotrice.* Par le DOCTEUR TOPINARD. Paris: J. B. Baillière et Fils. 8vo, pp. 575.

*On Motor Ataxia.* By DR. TOPINARD.

2. *Practical and Pathological Researches on the Various Forms of Paralysis.* By E. MERYON, M.D. London: Churchill and Sons. 8vo, pp. 215.
3. *On Paralysis, Neuralgia, &c., and their Successful Treatment by Galvanization, &c.* By JULIUS ALTHAUS, M.D. Third Edition. Small 8vo. Trübner & Co. Pp. 236.

THE disease which forms the subject of the first book on the list before us has been recognized but of late years; it had probably been observed by many in times gone by, but it had been absorbed in the general appellation of paralysis. To our great countryman, Dr. Robert Todd, is, however, due the merit of having first recognized it as a particular disease, and associated it with an organic change of the posterior columns of the spinal marrow.

The volume now before us forms a complete and most exhaustive monograph of "ataxie locomotrice progressive," or Duchenne's disease, as it is called by some; and no better proof of the intrinsic value of this book could be adduced than the fact of the French Academy of Medicine having awarded to it the Civrieux Prize in 1864. Commencing with the etymology of the word ataxia, Dr. Topinard defines it:—"A disorder of the functions which preside over progression, equilibrium, and other voluntary muscular acts; a disorder not caused by paralysis, nor muscular atrophy, nor by malformation of bones, &c., &c.; and totally different from trembling, chorea, and convulsions, whether general or partial, tonic or clonic." There is another form of ataxia which he cautions his readers against confusing with the real disease of Duchenne, a "muscular ataxia," the

causes of which are very different, as he says:—"The immediate causes of progressive motor ataxia are central, while its manifestations are peripheral; whereas muscular ataxia is a disease seated in the muscle itself, or in its motor nerve." A work like this, principally made up of cases carefully collected and lucidly described, and in most instances followed by highly interesting dissections, is about one of the most difficult to analyze or review satisfactorily. A great part of the book is taken up in endeavouring to point out what is, and what is not, genuine motor ataxia; for the author believes, and we agree with him, that this name has been at times very loosely applied to different forms of paralytic disease; and, after detailing a number of cases in which a condition resembling very much Duchenne's malady existed, he comes to the conclusion that, after all, the best test of this disease is the one originally pointed out by Romberg, viz., that the irregularity of motion in ataxia is only when the eyes are shut, while in other paralytic conditions it is the same whether the eyes are shut or open. Dr. Topinard defines this disease as follows:—"Progressive motor ataxia, according to the meaning generally attached to it at present, is a disorder of locomotion and equilibrium compatible with strict integrity of muscular force, and differing from tremblings, from chorea, and from convulsions." Having arrived at p. 143, he enters on the consideration of the commencement and progress of the disease, sometimes rapid, sometimes insidious—at one time reaching its acme in two years, at other times requiring twenty. Nothing could be better described than the history of the insidious approaches of this disease—the acute, flying, erratic pains, which so often and so erroneously are attributed to rheumatism—the derangement of sight, and the ophthalmoscopic appearances of the retina—the numbness, and the anesthesia. He then alludes to the appearances to be studied in cases of ataxia of the upper limbs, a disease much rarer than that which affects the lower limbs, and how to distinguish between it and general paralysis and tremor—a matter not so easy as might be thought at first sight. He then passes to the most important section, if not the most interesting portion, of the work, viz., what Dr. Topinard believes to be the most efficacious treatment. We regret to say that he holds out very small hope from medicines. We read that Duchenne's remedies, viz., iodide of potassium and electricity, have failed in his hands; and though derivations along the spine, tonics, and sometimes cupping between the shoulders, have given relief, still it is evident by his expression



that he places very little, if any, reliance in treatment, and hopes more from general hygienic measures. Alluding to nitrate of silver, as first employed by Wunderlich, and subsequently so much extolled by Charcot and Vulpian, our author acknowledges that in their hands it has proved of considerable efficacy in retarding the progress of the disease, and even in improving the condition of some patients; but still he seems to doubt its real value. Now, so far as we have had opportunities of studying this form of paralysis, we think ourselves justified in agreeing with Charcot and Vulpian in the advantages to be derived from the use of nitrate of silver, especially if administered during the early stages of the disease; and we know that Professor Banks, of this city, who has had ample opportunities, and has devoted much attention to this disease, holds the same opinion. But we must now take leave of Dr. Topinard, cordially recommending his book to those who wish thoroughly to make themselves up in "progressive motor ataxia."

Dr. Meryon's book, which we now take in hand, does not pretend to give us new views of either the physiology, the pathology, or the therapeutics of paralysis. It simply describes the different forms of this disease, with their *post mortem* appearances, together with the most approved mode of treatment in the present day. The book is divided into six chapters, the first being devoted to the structure and physiology of the nervous centres, and is illustrated with a very good plate representing a section of the spinal marrow. The cases, of which a great number are introduced as illustrations of the different forms of paralysis, have been very carefully selected from a variety of authors, both British and foreign, while the descriptions of each variety of paralysis is clear, though perhaps, in many cases, rather too concise. Progressive motor ataxia is, for instance, completely exhausted in *seven* pages. Still we must confess that the great characteristics of the disease are clearly laid down; the history of its invasion, slow progress, termination, and *post mortem* appearances graphically described. When discussing the treatment, however, he seems to ignore completely the use of nitrate of silver, which is the only remedy that in our hands has apparently been of any service.

The third book on our list is Dr. Althaus' little volume. We confess that, on reading its title-page, we became prejudiced against it. We fancied that it looked like a medical work for non-medical readers; but we changed our mind as we read it through, and we have no hesitation in recommending it, not as a work on galvanism



or electricity, but as a short compendious account of the different diseases in which electricity, in one form or another, may prove of service. In the first page Dr. Althaus describes, in a few words, the difference (so far as we know) between electricity, galvanism, and Faradaic electricity; and, having shown how much they differ in their several properties, occupies the rest of his manual with an enumeration of the cases in which electricity may be of use, together with the kind which should be employed in each case. Now, there can be little doubt that forms of electricity so different in their chemical action as galvanism and Faradaic electricity must, in all probability, be equally different in their action on the living frame; and we have little doubt that this powerful therapeutic agent has often been brought into disrepute in consequence of its having been injudiciously applied. Now, the little book before us will prove, we think, a convenient guide in selecting the form of electricity to be employed in any case which might present itself to a medical practitioner, and to prevent him from employing an interrupted current where he should have used a continuous one, or *vice versâ*.

1. *A Manual of Materia Medica and Therapeutics, including the Preparations of the British Pharmacopæia, and Many other Approved Medicines.* By J. FORBES ROYLE and FREDERICK W. HEADLAND. Fourth Edition. London: Churchill and Sons. 8vo., pp. 776. 1865.
2. *A Manual of Practical Therapeutics, Considered Chiefly with Reference to the Articles of the Materia Medica.* By EDWARD JOHN WARING. Second Edition. London: Churchill and Sons. 8vo., pp. 956. 1865.
3. *Therapeutics and Materia Medica; a Systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History.* By ALFRED STILLÉ. Second Edition. Philadelphia: Blanchard and Lea. 1864. Two Vols., large 8vo., pp. 1595.

THE works the titles of which head this article, are no new candidates for professional favour, each one and all of them having gone through the ordeal of at least one previous edition; in the case of

that by Messrs. Royle and Headland, this, indeed, being the fourth edition to which the work has reached; and yet this book in many respects presents all the features of a new, and, as it were, untried production; so extensive and radical are the changes rendered essential by the recent appearance of our long-promised, and, in many quarters, heartily-abused national Pharmacopeia; hence it is that it appears incumbent upon us to submit its pretensions to more careful examination than otherwise would seem to us to be necessary.

In the Preface it is stated that, "In this work the medicines of the British Pharmacopeia will be found arranged in natural order, the preparations described at length, and the formulæ explained." To a certain extent faith is kept with us, so far as these promises are concerned, but strict truth compels us to add that in many instances the explanations vouchsafed us of the processes are of the lamest and most meagre character. For instance, we read in the remarks upon the ferri phosphas of the Pharmacopeia, that

"The phosphate of soda (q. v.) contains 2 Equiv. of the Alkali with one of basic water. If added by itself to the solution of Sulph. Iron an iron salt of similar composition is precipitated ( $2\text{Fe O, H O, P O}_5$ ). This salt is white, but becomes blue on exposure. When the Acetate of Soda is added with the Phosphate, the precipitate is said to contain another Equiv. of Protoxide of Iron. There is some doubt, however, about the constitution of the Phosphate of Iron. According to Kane, the blue phosphate formed as above is a definite compound of the Proto-phosphate with the Perphosphate. But Gmelin has found the native phosphate, which it strongly resembles, to have the composition ( $3\text{Fe O, P O}_5 + 8 \text{ Aq.}$ )."

Now the object with which acetate of soda is introduced into this preparation is completely misunderstood by our authors; as we take it, the explanation given us in Macnamara's *Neligan* being the true solution of the reason why the pharmacopeial authorities introduced acetate of soda into this preparation:—

"EXPLANATION OF PROCESS.—Phosphate of soda consists of one equivalent of phosphoric acid in combination with two of soda and one of *basic* water (see p. 186). Phosphate of iron consists of three atoms of oxide of iron united with one of phosphoric acid; to furnish these three atoms of oxide of iron, three equivalents of sulphate of iron will be required; but phosphate of soda only contains enough

of base to saturate two out of the three equivalents of the resulting sulphuric acid, which would be objectionable inasmuch as the free sulphuric acid, exercising a solvent action over the phosphate of iron would be a source of loss in the process; an action not possessed by acetic acid: hence the necessity of employing the acetate of soda, one equivalent of which, with one of phosphate of soda, containing between them three equivalents of soda, saturate the three equivalents of sulphuric acid, forming three equivalents of sulphate of soda, which, with the acetic acid set free, are held in solution, whilst the three oxides of iron unite with the one phosphoric acid, forming the phosphate of iron, which is precipitated, thus,  $2\text{NaO}, \text{HOPO}_5 + \text{NaOA} + 3\text{FeOSO}_3 = 3\text{NaOSO}_3 + \text{A} + \text{HO} + 3\text{FeO}, \text{PO}_5$ . The precipitate is to be washed so long as the washings yield any precipitate with chloride of barium; in other words, until freed from sulphate of soda."

Again, as an example of loose writing, we may instance the explanation given us of the pharmacopeial process for the manufacture of the *zinci chloridum*:—

"The  $\text{H Cl}$  is decomposed, the  $\text{Cl}$  combining with the Zinc, and  $\text{H}$  gas being evolved. Iron is apt to be present as an impurity in the Zinc, and a chloride of iron would thus be formed at the same time. By the addition of Chlorine the Iron is peroxidized and converted into perchloride. Carbonate of Zinc being now added, the Iron salt is decomposed, and Peroxide of Iron separates, some more  $\text{Zn Cl}$  being formed, and  $\text{C O}_2$  escaping. The iron being separated by filtration, the Zinc salt is concentrated till ready to solidify on cooling, when it is cast into moulds for use as a caustic, and kept from the air on account of its strong attraction for the water of the atmosphere."

How, by the addition of chlorine, iron can become *peroxidized* is more than we can fathom; it certainly is converted from the state of proto to that of sesqui-chloride of iron, and is then, on the addition of the carbonate of zinc, peroxidized, but not until then. Our authors evidently have a just appreciation of the true nature of the case, but, in our opinion, such is not sufficient in a work that is so extensively read by our junior students; for them accuracy and precision are essential, and we think that we are doing our authors a service by drawing their attention to such omissions, even though rendering ourselves liable to the charge of being hypercritical.

Again, as an example both of meagre and inaccurate description, we

may adduce the following passage, purporting to be a correct explanation of the volumetric test given us in the Pharmacopœia for estimating the strength of no less important a medicine than prussic acid:—

“To ascertain this strength the B. P. gives a similar quantitative test. *3ss. of the acid treated with excess of solution of Soda, requires the addition of 80·66 measures of the volumet. sol. Nitrate Silver before a permanent precipitate begins to form, which corresponds to 2 per cent. of anhydrous acid.* When this quantity of the silver salt is added, the Cyanide of Silver is precipitated.”

Now the precipitate formed under the conditions stated is not, as incorrectly here asserted, *cyanide*, but *oxide*, of silver; and to enable our readers to estimate the *meagre* character of the proposed explanations we shall place in juxtaposition with it that given us in Macnamara's *Neligan*, where the nature of the test is correctly stated:—

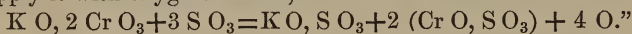
“This test is used for the purpose of estimating the amount of absolute hydrocyanic acid present in a given sample; for instance, in the Pharmacopœia it is stated that half a fluid ounce of the officinal acid, ‘when treated with an excess of solution of soda, requires the addition of 80·66 measures of the volumetric solution of nitrate of silver before a permanent precipitate begins to form, which corresponds to two per cent. of anhydrous acid.’ The rationale of this is, that oxide of silver is precipitated from a solution of nitrate of silver by a solution of soda, the soda abstracting the nitric acid to form nitrate of soda, and the oxide of silver being precipitated; thus,  $\text{AgONO}_5 + \text{NaO} = \text{NaONO}_5 + \text{AgO}$ . This latter forms, with cyanide of sodium a soluble double salt, cyanide of sodium and silver ( $\text{NaCy}, \text{AgCy}$ ); thus,  $2\text{NaCy} + \text{AgO} = \text{NaCy}, \text{AgCy} + \text{NaO}$ . The cyanide of sodium being produced in virtue of the action of the hydrocyanic acid upon the liquor sodæ; thus  $\text{NaO} + \text{HCy} = \text{HO} + \text{NaCy}$ ; so that no permanent precipitate can form so long as any cyanide of sodium is present in the solution. The moment it disappears the oxide of silver remains a permanent precipitate; and from this fact we judge of the entire disappearance of the cyanide of sodium, and the estimation of the per-centage of acid becomes but a simple matter of calculation. If one hundred measures of the volumetric solution correspond to 5·4 grains of absolute hydrocyanic acid, what are 80·66 measures equivalent to?  $100:5·4::80·66:4·35$ .



That is, half a fluid ounce of the solution contains 4·35 grains of anhydrous acid. But this only gives us the quantity of anhydrous acid present in the half ounce, not its *per-centage*; to ascertain this we must have recourse to another calculation; we must ascertain the number of grains in the half ounce, and that effected, we can easily by the rule of proportion arrive at its *per-centage*; to ascertain the number of grains by weight in a given measure of a fluid, it becomes necessary to multiply the number of grains contained in the measure by the specific gravity of the fluid; this will give us the number of grains by weight in the quantity operated upon; thus, the weight of a fluid half ounce of distilled water being 218·75 grains, that of a fluid half ounce of the officinal hydrocyanic acid will be  $218\cdot75 + \cdot997$  (its specific gravity) = 218·09375: which last quantity represents, as already shown, 4·35 grains of absolute acid, or as nearly as possible two *per cent.*, inasmuch as  $218\cdot09375 : 4\cdot35 :: 100 : 1\cdot99$ ."

Again, if we look to the explanation given us of the method adopted in the Pharmacopeia for making the sodæ valerianas, we find our authors equally at fault in their chemistry, their explanation being to this effect:—

"Valerianic acid ( $C_{10}H_9O_3, H O$ ) is derived from Fousel oil by the abstraction of 2 Eq. of Hydrogen, and the addition of 2 of Oxygen. Bichromate of Potash and Sulph. acid, being distilled together with the oil, supply it with oxygen. Thus,—



This error is more inexcusable than any of the preceding, inasmuch as reference to any standard work on chemistry would show that the action of sulphuric acid upon bichromate of potash is not what is here stated, sulphate of protoxide of chromium ( $Cr O, SO_3$ ) but sulphate of the sesquioxide of chromium ( $Cr_2O_3 \cdot 3SO_3$ ) *three* atoms, and not four, of oxygen being the result of the reactions; in fact the resulting salt being a species of chrome alum, the production of which is thus accounted for— $KO \cdot 2 Cr O_3 + 4 SO_3 \cdot HO = KO SO_3 + Cr_2 O_3 \cdot 3SO_3 + 4 HO + O_3$ .

To turn from the disagreeable duty of fault-finding to the more agreeable task of commendation, our readers are by no means to infer that all is barren in the work before us; far from it. Whilst we thus point out and condemn what is faulty in Messrs. Royle and Headland's book, and whilst in general terms we once for all repeat

that their explanations of the pharmacopeial processes are frequently incorrect, and, in most cases loose and meagre, and also that by far too little space is given to the more strictly therapeutical portion of their work, we are free to confess that most of their natural historical descriptions are good. As we have instanced passages to justify our censure, we may adduce the article on cinchona to warrant our commendation. What in all good faith we should advise our authors to do in a future edition of their work is to turn their attention to the correction and extension of those departments to which in this article we have taken exception, and as the result we will have a book far more worthy of the scientific reputation of its authors than is the present.

The second work on our list, that by Dr. Waring, is one the first edition of which frequently excited our surprise and admiration, that so valuable and so exhaustive a work could be produced by any gentleman placed under the adverse circumstances he must have been whilst writing it—quartered in a remote and solitary station in Burmah. Far from libraries, or other sources of professional aid, he compiled a work of which his *confrères* in the Indian army may well feel proud, as the production of the pen of one employed in the same service. In drawing attention to the second edition we may safely say that it more than supports the character earned for the work by its first edition. In depreciation of our author's labours it might be urged that the work is only one of compilation. We admit that fully, but not in depreciation. A more laborious, painstaking, and valuable composition does not exist in any other language on the subject—one that should be in every practising physician's and surgeon's study, without which their library, no matter how rich otherwise in works on *Materia Medica*, must of necessity be incomplete. We most cordially recommend it to our reader's notice, but in so doing it must be understood that it is to the man in full practice we commend it, not to the student. As a text book on *Materia Medica* it has no pretensions, nor has it been put forward as such by its laborious author, but to the practitioner it will be found of the greatest assistance.

The third work on our list is the second edition of a book, of the first edition of which we have already expressed ourselves in favourable terms. Like the last-named work it is eminently directed to the practitioner, being quite unsuited to discharge the duties of such

text books as Pereira's, Christison's, Neligan's, &c.; still it worthily discharges the duty to which it aspires. Less the mere record of other persons' experience than Waring's, it is written in a fuller and more professorial style. In our last notice of his work we had to take exception to some of Dr. Stillé's statements, statements which, we regret to perceive, continue in the present edition. Prominent amongst these are his observations on the plan of treatment of cholera by saline injections. These he repeats in the present edition without either modification or comment, a course of procedure which we consider unjust both to his readers and the gentlemen who have advocated, and, as they state, successfully employed, this plan of treatment. In a work professing to be an encyclopedia of therapeutics, no matter how strong may be the personal convictions of the author, he has no right to ignore, to pass over altogether—*sub silentio*—a line of practice which is stated to have been attended with such brilliant results. In most other instances, however, Dr. Stillé's work appears to be well written up to our present state of knowledge upon the subject, and we doubt not that it will long hold a high place amongst our standard works on therapeutics.

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*Medical Diagnosis.* By J. M. DA COSTA, M.D. Philadelphia, 1864. J. B. Lippincott and Co. 8vo., pp. 690; with Engravings on Wood.

THE names of Mott, Wood, Flint, and a host of able writers of general and special treatises on medicine and the allied sciences, occur to our minds when we take up for perusal a work by an American author; and, truth to say it is but seldom in late years that the ardent student of practical medicine, surgery, and midwifery has recourse to the pages of an American author without at once finding the profit of his labours. To this general panegyric we must, however, make marked exception in regard to the work now before us. What object it was originally designed to serve beyond that of enabling the writer to dub himself author of a work on Medical Diagnosis we cannot conceive. A work more barren and jejune it has rarely been our fate to pass through the ordeal of critical examination and review. In his Preface the author states that "his chief aim in writing this work has been to

furnish advanced students and young graduates of medicine with a guide that might be of service to them in their endeavours to discriminate disease." "I have sought to offer to those members of the profession who are about to enter on its practical duties a book on diagnosis of an essentially practical character—one neither so meagre in detail as to be next to useless when they encounter the manifold and varying features of disease—nor so full as to be unwieldy, and lacking in precise and readily applicable knowledge." Were we to seek for language in which to fully designate the special failings of this work, we need not pass beyond the author's own—shall we say anticipated, condemnation of his labour—as contained in the passage just cited. We open a page at random to look for those practical rules, which are to guide the student or the young graduate in medicine:—"Sun Stroke," p. 94, is disposed of in somewhat more than a page and a half; "Cataplexy," in less than a page; "Tetanus," in little more than three pages; "Chronic Hydrocephalus," in less than a page; and all this with such a vagueness in the detail of symptoms that we are at a loss to know whether the writer or his reader would be found in the more helpless state of ignorance and inefficiency for purposes of diagnosis, to say nought of treatment, after maturest study of the several sections referred to. The various subsequent sections on "Diseases of the Lungs," "Diseases of the Heart," "Diseases of the Abdomen," are disposed of in an equally cursory and superficial manner. To the "student" who has read the ordinary text books the work is absolutely useless—to the "young graduate of medicine" in search of an aid to diagnosis in a difficulty of practice, it is worse than waste time and labour to have recourse to such a bald and often unconnected statement of a few leading symptoms. The chapters on Diseases of the Stomach—on Renal Affections—on Fevers, equally betray a mind wanting in all the essentials of medical graphic power. There is nought that speaks of that skill and knowledge to be got alone from actual contact with disease. Authors are cited, it is true, but with scant appreciation of their labours or the ends and aims in medicine they have worked for.

In fine, we lay down this work with as much regret and pain that it should ever have been produced, as it is well possible to feel on the part of a critic and reviewer. Not by such writings is medicine to be advanced.



1. *An Inquiry into the Relative Frequency, the Duration, and Cause of Diseases of the Skin, as deduced from the Observation of One Thousand Consecutive Cases; with Remarks on the Exanthematous Epidemic of the Spring of 1864.* By ERASMUS WILSON, F.R.S. London: Churchill and Sons, 1864.
2. *The Student's Book of Cutaneous Medicine and Diseases of the Skin.* By ERASMUS WILSON, F.R.S. London: Churchill and Sons. Part I., 1864. Part II., 1865.
3. *Handbook of Skin Diseases for Students and Practitioners.* By THOMAS HILLIER, M.D., Lond., &c. London: Walton and Maberley. 1865.
4. *On Psoriasis and Lepra.* By T. M'CALL ANDERSON, M.D., &c. London: Churchill. 1865.

IN the number of this Journal for August, 1864, we had occasion to review works on skin diseases by Blake, Caillault, Anderson, and Frazer; and subsequently, in the number for February in the current year, we reviewed at some length the recent work of Dr. Tilbury Fox on the same subject. Such being the case we do not now propose to weary our readers with a repetition of any remarks made in our former notices; nor from the length and variety of the works above grouped together will it be possible for us to give more than brief notices of some particular points in each of the volumes now before us.

It is a subject of general remark that of late years the human skin has largely attracted the attention of London practitioners. A few years ago a good book on the subject was rare; now, however, they pour from the press with astonishing rapidity, and, we must add, with greatly improved tone as regards their matter and manner. In a word, they are *bonâ fide professional* books. Whether they are published simply in the ardent pursuit of medical science, or with a view to competition for the throne of cutaneous practice which sooner or later must be vacant, it is not for us to say. The fact, however, remains, that science profits by this dermatological plethora, although it is equally true that of making books there is no end, and that the study of the most of them is verily a weariness to the flesh. Any comparison of the works now before us is

out of the question. Mr. Wilson and Dr. Hillier differ materially on the matter of classification; and where names common to both writers are not the signs of similar ideas comparisons are simply impossible.

The title of the first book above noted sufficiently explains its object, and the statistics contained in it are chiefly valuable because they are collected from cases treated by one practitioner, and that one *the* man whose experience is of more authority on this subject than that of any other man in this country.

The cases here enumerated are those occurring in middle and upper class private practice, and are altogether exclusive of the more numerous class to be met with in hospitals, dispensaries, and work-houses.

In 1,000 cases, registered under these circumstances, there occur fifty-one different diseases, which are thus arranged in order of frequency:—

Eczema, . . . . .	298	Kelis, . . . . .	5
Gutta rosacea, . . . . .	112	Hirsuties, . . . . .	5
Alphos, . . . . .	73	Urticaria, . . . . .	4
Acne, . . . . .	55	Kérion, . . . . .	4
Alopecia, . . . . .	50	Leucosma, . . . . .	3
Pityriasis, . . . . .	43	Ecthyma, . . . . .	3
Trichosis, . . . . .	39	Nævus hypertrophicus . . . . .	3
Scabies, . . . . .	37	Purpura, . . . . .	2
Lichen, . . . . .	30	Sudatoria, . . . . .	2
Area et calvities, . . . . .	30	Cachexia cutis, . . . . .	2
Syphiloderma, . . . . .	30	Tumores encystici, . . . . .	2
Erythema, . . . . .	17	Molluscum, . . . . .	2
Chloasma, . . . . .	15	Morbi unguium . . . . .	2
Sycosis, . . . . .	14	Canities, . . . . .	2
Furunculus, . . . . .	14	Erysipelas . . . . .	1
Prurigo et pruritus, . . . . .	13	Pemphigus, . . . . .	1
Lupus non exedens, . . . . .	12	Roseola, . . . . .	1
Scrofuloderma, . . . . .	11	Hordeolum, . . . . .	1
Herpes, . . . . .	11	Narcosis folliculorum, . . . . .	1
Melasma, . . . . .	8	Favus, . . . . .	1
Lupus erythematosus, . . . . .	7	Verruca, . . . . .	1
Impetigo, . . . . .	6	Atrophia cutis, . . . . .	1
Xeroderma et ichthyosis, . . . . .	6	Morphœa, . . . . .	1
Nævus vasculosus, . . . . .	6	Elephantiasis, . . . . .	1
Lupus exedens, . . . . .	5	Bucnemia, . . . . .	1
Carcinoma cutis, . . . . .	5		

Mr. Wilson gives the foregoing numbers, divided according to what he calls a CLINICAL CLASSIFICATION, thus:—

1 Eczematous affections, . . . . .	526	14 Carcinomatous, . . . . .	5
2 Erythematous, . . . . .	23	15 Leprous, . . . . .	2
3 Bullous, . . . . .	12	16 Affections of the hair and	
4 Furuncular, . . . . .	17	hair follicles, . . . . .	146
5 Nervous, . . . . .	13	17 Affections of the sebiparous apparatus, . . . . .	60
6 Vascular, . . . . .	6	18 Affections of the Chromatogenous, . . . . .	27
7 Hæmodyserasic, . . . . .	2	19 Affections of the Sudoriparous, . . . . .	2
8 Developmental and nutritive	8	20 Affections of the nails, . . . . .	2
9 Hypertrophic and Atrophic,	6	21 Phytodermic affections, . . . . .	73
10 Alphous, . . . . .	73		
11 Strumous, . . . . .	35		
12 Syphilitic, . . . . .	30		
13 Keloid, . . . . .	5		

Now it is obvious that the value of this latter set of figures is greatly diminished in the eyes of the many who do not accept Mr. Wilson's "Clinical Classification" when we mention that he regards pityriasis and scabies as eczematous affections; so that when he says he met with 526 cases of eczematous affections in 1,000, he means only that *he* classes them as eczematous, not that they will be generally regarded as such. The same remarks apply to including cases of psoriasis in the 526; for in his Student's Book he looks on psoriasis as a species of eczema.

Having thus stated the sense in which Mr. Wilson uses the term eczema, we find from his researches that the age most fertile in the production of this class of diseases is that of maturity and incipient decline, ranging between the ages of 30 and 59, while the next most productive period is childhood and youth. The chronic nature of this class he also shows when he informs us that of the 526 cases 29 lasted for one month, 89 from one to six months; 30 from 6 months to 12; 85 from 1 to 5 years; 27 from 5 years to 10; 23 from 10 to 20 years; 12 from 20 to 30 years; while in three instances the disease lasted for the respective periods of 48, 57, and 70 years. Mr. Wilson looks on *debility* as the most general cause of eczema, and he divides debility into *nutritive*, *nervous*, *assimilative*, and *local*. We think his distinction between *nutritive* and *assimilative* debility is one which cannot be sustained; to us it seems that the one is implied in the consideration of the other, and we cannot understand what is meant by the writer on this part of his subject. Throughout

80 pages of large 8vo Mr. Wilson pursues each of the twenty-one groups in his clinical classification; dividing them into species, and separate diseases, and giving no end of figures as to their relative frequency and duration as to age and sex. Regarding the *causes*, we do not see that he establishes a clear proof of their connexion with the diseases, and the most that can be allowed is to admit his statements as *probable* evidence. It is, doubtless, plausible to state that debility of some kind is the chief cause of eczema, because it may be shown to exist in most cases; but to *demonstrate* the proof of connexion as to cause and effect is quite another matter.

Statistically this book is a valuable one, and will be chiefly useful to writers on cutaneous medicine, who will find ready-made calculations which may be safely given on the authority of so eminent a man as Erasmus Wilson, F.R.S.

The Student's Book of Cutaneous Medicine professes to embrace the entire subject of dermatology, and to place it in the best way before the student. It consists of two volumes, and the arrangement of the work is based on the clinical classification to which reference has already been made. It is not set down as in the former treatise, and in order to give a fair idea of the work now before us we shall give it in *extenso*. It consists of twenty-two groups, thus:—

1 Eczematous	affections	13 Zymotic	affections
2 Erythematous	"	14 Syphilitic	"
3 Bullous	"	15 Leprous	"
4 Furuncular	"	16 Pigmentary	"
5 Nervous	"	17 Phytodermic	"
6 Vascular	"	18 Ungual	"
7 Hæmic	"	19 Diseases of the hair system	
8 Developmentaland nutritive,,		20 Diseases of the sebiparous	
9 Hypertrophic and Atrophic,,		system	
10 Alphous	"	21 Diseases of the sudoriparous	
11 Strumous	"	system	
12 Carcinomatous	"	22 Traumatic affections	

Pages 1–46 treat of the Anatomy and Physiology of the Skin; and pages 47–65 treat of its Pathology and of the Classification of its Diseases. Under the last head of course Mr. Wilson's Clinical Classification receives full explanation and elucidation; while the student is also given Plenck's Classification of 1776, Willan's of



1798, Alibert's of 1810, Hardy's, and (in the Appendix to Vol. II.) the now celebrated arrangement of Professor Hebra, of Vienna.

We shall take Chapter III., on Eczematous Affections, as a sample of the whole work. Here we find some very badly matched relatives which, in our view, have been wisely divorced in other systems. Eczema, psoriasis, pityriasis, lichen, impetigo, scabies, and gutta rosacea are all here given under the above head. We have already entered a protest against classing together psoriasis, and pityriasis as eczematata; and we must also include scabies. This last is entirely inconsistent with the idea on which Mr. Wilson's system is based, for it cannot be pretended that the classing of scabies as an eczematous affection gives a true idea of the nature of that disease. Further we find eczema described under various pathological forms. Thus, under the head "Regular," we have Eczema Erythematosum, E. Papulosum, E. Vesiculosum, &c.; under the head "Irregular Forms" we have E. Fissum, E. Sclerosum, E. Venucosum, &c.; under the head "Forms of Distribution" we have E. Universale, E. Figuratum, E. Diffusum, &c.; and under the head "Local Forms" we have E. Capitis, E. Faciei, E. Aurium, &c., &c. In this class of diseases Mr. Wilson looks on arsenic as an infallibly curative agent. He recommends its use in all ages; for infants under two years he would prescribe doses of one minim of Fowler's solution thrice daily.

Mr. Wilson writes on a very common disease with a seeming accuracy of description which our personal experience is unable to confirm, or offer any opinion on. He says (p. 121), "The pruritus of scabies is somewhat different from that of other pruriginous complaints; it is a kind of tickling itching, and the rubbing and scratching employed to relieve it are said to be pleasurable. King James I. has the credit of having declared that none but kings and princes should have the itch, for the sensation of scratching was so delightful." From all we have seen of this interesting malady, popularly known in this country as "the Scotch fiddle," we are inclined to think that were King James now living his itchy subjects would gladly make over the royal disease to their sovereign liege. Chapter IV., on "Erythematous Affections," treats of erythema, erysipelas, urticaria, and roseola. Mr. Wilson here restores erysipelas to the place from which it has been of late rejected by writers on cutaneous medicine, who have mostly ceased to regard it as a skin disease. Chapter V., on "Bullous Affections," treats of herpes, miliaria, and pemphigus. Chapter VI.,

on "Furuncular Affections," deals with furunculus, hordeolum, anthrax, and ecthyma. Chapter VII., on "Nervous Affections," has included in it four groups, hyperesthesia, anesthesia, pruritus, and prurigo. Chapter VIII., on "Vascular Affections;" chapter IX., on "Hæmodyscrasic Affections;" Chapter X., on "Developmental and Nutritive Affections," and Chapter XI., on "Hypertrophic Affections," conclude the first volume, which space does not admit of our discussing in detail. It is one of the best books we have seen on the subject.

The second volume of Mr. Wilson's work consists of thirteen chapters (XII.–XXIV.); an appendix containing Hebra's Classification, and a copious index to the entire work. The titles of these chapters are as follows:—XII., Alphous Affections; XIII., Strumous Affections; XIV., Carcinomatous Affections; XV., Zymotic Affections; XVI., Syphilitic Affections; XVII., Leprous Affections; XVIII., Pigmentary Affections; XIX., Phytodermic Affections; XX., Ungual Affections; XXI., Affections of the Hair System; XXII., Affections of the Sebiparous System; XXIII., Affections of the Sudoriparous System; and XXIV., Traumatic Affections. Of these we particularly recommend the perusal of Chapter XV. (Zymotic Affections), specially observing the remarks on Vaccinia, at p. 353; of Chapter XVIII. (Pigmentary Affections); Chapter XXI. (Affections of the Hair System); and Chapter XXIV., on Traumatic Affections. In Chapter XII., p. 286, he makes the following practical remarks:—"The best form for the administration of arsenic is the liquor potassæ arsenitis, or Fowler's Solution. Another good form is that of De Valangin, the liquor arsenici chloridi; and, occasionally, we may have recourse to Donovan's Solution, the liquor hydriodatis hydrargyri et arsenici; commencing with the first, we may prescribe a mixture which we believe cannot be excelled, as follows:—℞ vini ferri, ʒi ss; syrupi simplicis, ʒij; liquoris, Fowler, ʒij; aquæ puræ, ʒij; the dose to be one drachm three times a day, and taken in the middle of a meal. The patient should be furnished with a minim measure, and, after measuring the dose, he should drink it pure, out of the measure. The reasons for these instructions are, that where a medicine is to be taken for many months, and three times every day, it is important that it should be as agreeable to taste as possible; and also that it should be administered in the smallest dose practicable; again, a small quantity is less likely to nauseate the stomach than a larger one.

Secondly, by taking the dose in the middle of the meal, it is secured a more thorough admixture with the food; and it is less likely to be brought into direct contact with the mucous surface. We may add that the experience of many years has convinced us that this is the best, and therefore the only method according to which this very important remedy should be used."

In tubercular syphilis, particularly in the chronic tubercular, and superficially ulcerating forms, and also in syphiloderma palmæ et plantæ, Mr. Wilson advises the following formula (p. 374):—"decoctum Zittmanni fortius."  $\mathcal{R}$  radicis sarsæ concisæ  $\mathfrak{z}$  xij, aquæ fontanæ libr. lxxij. Digest for twenty-four hours; then add, tied up in a piece of linen:—sacchari albi, aluminis āā  $\mathfrak{z}$ vj; calomelanos,  $\mathfrak{z}$ iv; hydrargyri bisulphureti rubri (cinnabar),  $\mathfrak{z}$ j; simmer down to 12 quarts; towards the close of the simmering, add seminum anisi contus; seminum fœniculi contus āā  $\mathfrak{z}$  ss; foliorum sennæ,  $\mathfrak{z}$ ij; radicis glycyrrhizæ, concis,  $\mathfrak{z}$ iss. Press and strain, and, after standing until cool decant the clear liquid, and bottle 12 quarts." Of this decoction one *quart* is to be taken warm before noon daily.

On page 450, Mr. Wilson, discussing favus, announces his view that it is not a parasitic disease; and this he does because he does not recognize contagion as a means of communicating the affection. We cannot any further discuss the merits of this remarkable work, which we have no hesitation in pronouncing the *most useful* of the many written by Mr. Wilson, and we cordially recommend it not merely to students, but to experienced practitioners also.

Dr. Hillier's book is quite a different article from those of Mr. Wilson. Making a clean sweep of all English books previously written on his subject, he gives us this, his own production, because—"The existing books were either out of date, diffuse, inaccurate, or incomplete." As the reader proceeds he finds a general acknowledgement of the works of Hardy and Hebra, of which it seems Dr. Hillier has "largely availed" himself. Dr. Hillier adds:—"I have also obtained information from the work of Dr. Gustav Simon, from Dr. Jenner's Lectures, from MM. Bazin and Devergie's writings, and from Dr. Thompson's Treatise, edited by Dr. Parkes."

Notwithstanding all this assumption of superiority, we are bound to say that Dr. Hillier's book—always excepting the Preface—is a good one, and, like many of the best things in this world, is better than it looks. Of course he has "the author's classification," as well

as any other writer; but it is, in our opinion, much better than most others, because, to some extent, it adheres to Willan's principle of elementary lesions, while it partly proceeds on the supposed natural affinities of the diseases. Contrary to the opinion of Mr. Erasmus Wilson, Dr. Hillier believes in the parasitic nature of favus; he also restores the exanthemata to the category of skin diseases, and gives a very useful tabular view whereby the student may readily fix in his memory the diagnostics of this class of diseases. Chapter XIII., in which he treats of Morbus Addisonii, is particularly worth reading; and so are his remarks on prurigo, psoriasis, eczema, and vaccination. There is a good deal of bookmaking in the tedious records of cases which in a work of this kind may well be omitted. On the other hand there is a very useful formulary at the end of the volume, and there are two good plates giving microscopic views of morbid growths in tinea and other affections. These views are magnified 200 diameters. His style is concise and manly, and if most readers follow the common practice of reading the Preface last, they will agree with us when we commend Dr. Hillier's observation and industry, and wish every success to this really good book.

In our number for August, 1864, we reviewed Dr. M'Call Anderson's Treatise upon Eczema, and on it we remarked:—"Dr. Anderson's book is a good one, and we recommend its perusal to such as feel a special interest in its subject."

The present volume (No. 4 in our list) is a monograph, and forms the third of Dr. M'Call Anderson's *Contributions to Dermatology*. It consists of 62 pages 8vo, printed on tinted paper, and illustrated with a well executed coloured lithograph of what the author terms "Psoriasis Rupioïdes."

Space will not admit of our noticing more than a few points in this work.

In the first place, then, the author repudiates any difference between lepra and psoriasis. He looks on the former as merely one of the declining stages of the latter, and in this view we fully agree with him. His view is a practical one; and though our readers will recollect that it was held by the late Dr. Neligan, yet it must be borne in mind that Dr. M'Call Anderson opposes the doctrine of no less an authority than Erasmus Wilson. The coloured lithograph, already referred to, is illustrative of a variety which we shall let the author describe for himself:—"There is a peculiar appearance which psoriasis sometimes assumes, which I first observed a



good many years ago, and which has never been described. When it occurs, it may be regarded as a stage intervening between the so-called psoriasis guttata and nummularis. In it the accumulation of epidermis takes place to an unusual extent, so that on many of the patches it assumes the shape of large conical crusts marked by concentric rings. In fact, they exactly resemble in shape limpet shells, and from their likeness to crusts of rupia, I have called this variety psoriasis rupioides. Except in the shape of the crusts, however, there is no connexion whatever with rupia, and on removing a crust there is no ulceration beneath, but a slightly elevated dusky-red rounded surface is exposed to view, which sometimes bleeds a very little" (p. 4.) On p. 6 Dr. M'Call Anderson remarks that itching in psoriasis, if at all present, is moderate; and on p. 12 he states that psoriasis is rare in strumous persons. Pp. 32-35 are occupied with very useful tables of diagnosis. Thus we have a comparative view of syphilitic and non-syphilitic psoriasis; the diagnosis of dry chronic eczema from psoriasis; of pityriasis from psoriasis; of pityriasis rubra acuta, and of herpes circinatus from psoriasis. As regards treatment, he specially recommends the local application of oleum rusci obtained from the bark of the white birch. This monograph winds up with a humorous description of the baths at Leuk, from one of Murray's Hand-books, for the truth of which Dr. M'Call Anderson personally vouches. We cordially recommend this book to practical men, to whom it will doubtless prove most useful; and we hope next year to have the pleasure of reviewing a *fourth* monograph of Dr. M'Call Anderson.

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*On the Temperature of the Body as a Means of Diagnosis in Phthisis and Tuberculosis.* By SYDNEY RINGER, M.D., &c. London: Walton & Maberly. 1865.

WE have risen from perusing this book with feelings of no small satisfaction. Its author is an original observer; and he shows considerable lowliness of self esteem when he terms the result of much observation and tedious experiment "this pamphlet." Dr. Ringer is Professor of Materia Medica and Therapeutics at University College, Assistant Physician to University College Hospital, and Assistant Physician to the London Hospital for Sick Children. From the two latter positions he has derived much of the experience exhibited "in this paper;" the observations recorded

in which were obtained from patients under the care of Drs. Jenner, Hare, and Reynolds.

The latter and greater portion of this small volume is occupied with minute details of cases, to some of which the temperature is appended in the form of elaborate charts.

Dr. Ringer remarks that the temperature of the body should be taken by a thermometer placed in the axilla; that the patient should be in bed, and undressed for an hour before the temperature is taken; that he should be placed diagonally on the right or left side; and that he should have been previously covered up, not exposing the axilla, lest a difference of 2° or 3° Fahr. may result. If the patient has been lying on one side he should be turned diagonally on the other, so as to use the axilla which was previously most dependent. Care should be taken that the thermometer be in complete contact with the skin, and that no clothes are in the way to separate it from the surface of the body. The thermometer should remain in the axilla for at least five minutes; and it should be always tested, as even those sold by the best makers sometimes vary so much as 1° Fahr. The temperature should be taken at 8 a.m. and 8 p.m. daily, or, if only once, at the evening hour when it is elevated as compared with the morning. The preceding remarks contain the substance of Dr. Ringer's directions for the use of the thermometer; his *conclusions*, which he afterwards considers as propositions *seriatim*, are as follow:—

“1. There is probably a continued elevation of the [temperature of the] body in all cases in which a deposition of tubercle is taking place in any of its organs.

“2. This elevation of the temperature is probably due to the general condition of the body [tuberculosis], or to the deposition of tubercle in the various organs [tuberculization].

“3. This elevation is probably due to the general condition [tuberculosis] rather than to the deposition of the tubercle [tuberculization].

“4. The temperature may be taken as a measure of the amount of the tuberculosis, and tuberculization, and any fluctuations in the temperature indicate corresponding fluctuations in the severity of the disease.

“5. The temperature is a more accurate indication of the amount of tuberculosis and tuberculization than either the physical signs or the symptoms.

“6. By means of the temperature we can diagnose tuberculosis and tuberculization long before the physical signs and symptoms are sufficient to justify such a diagnosis.

"7. By means of the temperature we can diagnose tuberculosis even when, during the whole course of the disease, there are no physical signs indicative of tubercular deposit in any of the organs of the body, and in which cases the symptoms (apart from the temperature) are inadequate to enable us to arrive at such a diagnosis.

"8. It is probable that by means of the temperature we can conclude that the deposition of the tubercle has ceased, and that any physical signs that are present are due to obsolescent tubercle and the chronic thickening of the lung tissue between the tubercular deposit.

"9. It is probable, though further observations on this point are necessary, that the temperature of the body affords a means by which we can diagnose between diseases in which the symptoms and physical signs are either too scanty or too much alike to enable us to decide between them."

The details of Dr. Ringer's cases, and of others quoted by him, go to show that where phthisis or tuberculosis is present the temperature is abnormally high, from 100° to 105° Fahr.; that secondary complications did not sufficiently account for the elevation of the temperature; and that by means of temperature tuberculosis and tuberculization can be diagnosed at an early stage, long before the detection of physical signs or the appearance of symptoms. What is particularly worthy of note, and what we desire to put prominently before our readers, is the exact manner in which Dr. Ringer removes from the case all those causes or morbid conditions which might be held to influence, in any way, the result of his observations. In fact he seems to have taken every precaution to make his observations as precise as any such can be in the absence of mathematical demonstration.

It is scarcely necessary to point out the very great practical importance of the temperature of the body as a means of diagnosis in phthisis and tuberculosis. Not only does Dr. Ringer show it to be a more accurate indication of the *amount* of tuberculosis and tuberculization than either the physical signs or the symptoms; but he also shows that the advent of phthisis can, by this means, be discerned afar off, while there is yet time for medical treatment or change of climate.

The book is a little one, only 92 pages of small 8vo., in all; but we strongly recommend every physician to get it. For substantial value, clear and concise statement, and acute observation, it has few equals in this weary age of much study and many new books.

*A Practical Essay on the Use of the Nitrate of Silver in the Treatment of Inflammation, Wounds, and Ulcers.* By JOHN HIGGINBOTTOM, F.R.S., Hon. Fell. R.C.S., Eng. Third Edition. London: Churchill and Sons. 1865.

IN the introduction to this volume Mr. Higginbottom, of Nottingham, states that his attention was first directed to the use of nitrate of silver about forty-five years ago. Accordingly we are to look in this volume for the results of a very lengthened experience; nor will the reader find his expectations disappointed in this respect. Like most riders of hobbies, Mr. Higginbottom seems to look on his favourite remedial agent as a panacea for all the ills of the flesh, just as Mr. Hunt looks on arsenic, and as the late Dr. Todd, to some extent, regarded alcohol; but, it must be admitted, that the large number of detailed cases which fill up most of his book go far to establish his theory to a greater extent than is generally admitted by the majority of practitioners. *In limine*, Mr. Higginbottom announces his belief that nitrate of silver is *not* a caustic or corrosive substance, but, on the contrary, that it possesses a peculiar *conservative* property, calculated to preserve, and not to destroy, the animal tissue to which it is applied. He recommends the formation of an eschar, which becomes "adherent and hard like a piece of black sealing-wax;" and concludes that when an eschar of this kind forms over a recent wound, the latter, under ordinary circumstances, invariably heals.

Cases are detailed to show, that by the timely application of his favourite remedy, the direful effects of punctures from needles, nails, hooks, thorns, bayonets, saws, &c., are totally prevented; that in lacerated wounds the consequent irritation and inflammation are prevented or removed; in recent contused wounds the inflammation is subdued, and consequent suppuration and loss of substance is prevented; while in phlegmonous inflammation the progress of tumefaction and inflammation is arrested, and suppuration is often prevented.

Mr. Higginbottom does not believe erysipelas to be constitutional; and his chief reason for believing it to be a local affection is the result of his practice, which satisfied him that the application of nitrate of silver was all-powerful. Neither does he believe in the doctrine of metastasis, because, during a long experience, he has never met with a single case resulting from the dreaded use of



nitrate of silver. That erysipelas is simply a local affection we do not believe Mr. Higginbottom has proved; indeed the *sensus communis* of the profession is so reasonably opposed to him on this point that we need not here discuss the question; but, we may remark that the success or failure of the use of nitrate of silver as a local application cannot vitally affect the question at all. When his remedy has been used, he believes that in simple acute erysipelas, the inflammation is immediately arrested, and quite subdued in four days; that when affecting the face and scalp, cerebral irritation is invariably prevented, and delirium ceases. Further, he is of opinion that in phlegmonous erysipelas the inflammation is often arrested, and so far subdued, as to prevent extensive sloughing of the cellular tissue; or that, if suppuration takes place, it is similar to phlegmon. The nitrate of silver is recommended to be used in so strong a solution as twenty grains to the fluid drachm; and in pp. 16–19 full directions are given as to the mode of its application under all circumstances.

Reference is also made to its use as a means of inducing the healing process, or adhesive inflammation; as a means of healing by eschar; also to its use in whitlow, variola, gangrena senilis, and destructive inflammation of the eye.

Perhaps the best part of the volume is that consisting of Chapters VI., VII., and Appendix. Chapter VI. is on the treatment of ulcers, and in it are precise directions for the use of the nitrate of silver in small irritable ulcers, with varicose veins; “on healing small ulcers by the unadherent eschar;” “of ulcers attended with inflammation;” “old ulcers of the leg,” &c. The general conclusion arrived at is—that in ulcers the inflammation is removed, and the healing process greatly facilitated by the use of nitrate of silver; and that these effects are very apparent in the treatment of phagedenic ulcers.

In Chapter VII., “of burns and scalds,” Mr. Higginbottom observes:—“In the first class of burns and scalds, where there is superficial inflammation, and in the second, where there is simply vesication, without destruction of the cutis vera, the application of the nitrate of silver . . . is often a speedy remedy. The vesicles should be removed, and the nitrate of silver, either in the stick or concentrated solution, should be slightly passed *once* over the burnt or scalded surface.” He then states that the application of the nitrate does not appear to increase the pain of recent burns or scalds, but rather the reverse; and that, while it excludes

the air and prevents sloughing, the healing process is generally facilitated by exposure of the parts to the open air.

In the treatment of ulcers he uses "Kirkland's neutral ointment."<sup>a</sup>

He finds this cerate, spread on linen, very useful, "as a defence, after using the nitrate of silver." In ulcers, with slight discharge, he uses an absorbent preparation, under which an adherent eschar is formed. This preparation is called "black lint," and is made—by saturating an ounce of fine lint in a solution of nitrate of silver, two drachms to four ounces of distilled water, and then exposing it in a flat shallow vessel to dry by evaporation.

We must not omit the following conclusions resulting from the application of nitrate of silver:—

"In large incised wounds, when the interrupted suture is used, inflammation and suppuration are prevented, and the wound heals by the first intention.

"In variola, the early pustule is immediately arrested in its progress, and the pitting of small-pox is entirely prevented.

"In dissection wounds the specific poison is neutralized, and rendered innocuous, so that all danger is prevented."

While general experience is in favour of the third of these conclusions it certainly is not so with the first and second. In the Appendix there are remarks "on the use of nitrate of silver as a blister;" and on its use in cases of gunshot wounds, neuralgia, contraction of the rectum, ulceration of the tongue, irritable ulceration near the eye, fungous ulcer of the navel in infants, and chilblains.

We have now endeavoured to give the reader a brief outline of the scope and contents of these 172 pages of large 8vo. That all Mr. Higginbottom's conclusions will be accepted by the profession we do not believe, but that many of them have been long so, and that all deserve a full consideration, we are quite certain. As a record of long experience, and of a belief, strengthened by years of untiring observation, we cordially commend this volume to the notice of the profession.

<sup>a</sup> R Emp. plumbi, ʒvi.; olei olivæ, ʒiii.; cretæ ppt., ʒiv ss.; aceti distillati, ʒiv. Mix the acid and chalk in a mortar, and add the oil and lead plaster, previously melted together; stir all until cool, and keep under a little water.

*Photographs of Eminent Medical Men of all Countries, with brief Analytical Notices of their Works.* Edited by T. H. BARKER, M.D., F.R.S., Edin., &c. The photographic portraits from life by ERNEST EDWARDS, B.A. London: Churchill.

WE have received three numbers of this serial, each containing three photographs, with short memoirs, of members of our profession, and we hail them with pleasure. The wish to know something of the personal appearance and history of those who have distinguished themselves, or with whose teachings and writings we have been familiar, is not unnatural. In the ranks of literature and science this wish has been very freely gratified, and we see no reason why it should not be so in medicine also. It is true, contemporary biography may be abused; but to forbid the use of that which is good, because it may be abused, is to show a consciousness of very great weakness. Some years ago an attempt was made by a few to unduly advertise themselves by the publication of their likenesses and biographies, but the voice of the profession was quite sufficient to put that down, as, no doubt, it will again if necessary. In the meantime let us enjoy the goods provided for us.

The photographs are admirably executed, and the memoirs are written with good taste and much care.

## PART III.

### MEDICAL MISCELLANY.

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*Reports, Retrospects, and Scientific Intelligence.*

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#### PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.<sup>a</sup>

DR. CHURCHILL, President.

*Disease of the Mitral Valve.*—DR. HAYDEN exhibited a specimen of disease of the heart, consisting of contraction of the mitral orifice and dilatation of the right auriculo-ventricular opening, together with a typical example of pulmonary apoplexy. They were taken from the body of a female, aged twenty-eight, who was admitted into the Mater Misericordiæ Hospital on the 24th of November last, and died on the 26th of January. She was a seamstress by trade, and had never had rheumatism; but he learned from her that about three years ago she complained, for the first time, of palpitation of the heart, with occasional breathlessness on making any unusual exertion. A year before she came under his observation she had cough for the first time. During four months antecedent to the time of her admission she was unable to lie down comfortably in bed, owing to the dyspnea which this change of posture produced. One month previously her feet began to swell. Her face was rather florid, with a little puffiness about the eyes; there was no lividity or congestion of the lips or of the extremities; the pulse was rapid, exceedingly weak and faltering; there was œdema of the lower extremities, great enlargement of the cervical veins, and there was a very remarkable pulsation of the jugular veins synchronous with the systole of the ventricles; the slightest pressure immediately above the clavicle stopped this pulsation; the chest was unusually resonant, and permanently distended as in inspiration; the impulse of the heart was rather strong. On examination with the stethoscope he detected the first sound, well pronounced, but somewhat rough, and immediately preceding it there was a rough murmur, loudest over the apex; it did not at all implicate the first sound. There was a murmur audible over the

<sup>a</sup> These reports are furnished by Dr. R. W. Smith, Secretary to the Society.



centre of the sternum with the first sound—a decided systolic murmur, of a soft character, not at all like the rough murmurs, and confined to the region of the right ventricle. The second sound was clear, but was distinctly reduplicated; the reduplication being audible over the middle of the sternum, where the systolic murmur was found. He had no difficulty in diagnosing from these signs and symptoms mitral contraction; and he further ventured to say that they should find hypertrophy of the left auricle, with dilatation of the right cavities and right auriculo-ventricular opening, and emphysema of the lungs. It was unnecessary to detail the further history of the case, more than to state that on the 17th December there was hemoptysis; the blood was florid, and mixed with frothy mucus. He now learned, for the first time, from the patient, that she had repeatedly before spat blood, but never in such quantity as on this occasion. On the 31st December she complained of acute pain in the region of the heart, extending to the left scapula and down the left arm. This pain, with the accompanying respiratory distress, was of a decidedly anginal character. On the 11th of January, having examined the patient carefully from day to day, he requested his colleague, Dr. Hughes, to see the case with him. It was subsequently found that, in addition to the presystolic murmur, there was a well pronounced murmur accompanying the first sound, and constituting, as it were, a continuation of the former; thus they had two murmurs converted into one. On the following day he found that the presystolic murmur had ceased to be audible, and the systolic murmur was alone heard. A week before the patient's death both radial pulses became imperceptible. She was still able to creep about the ward, and, notwithstanding her extreme debility, declared she felt herself improved. Two days before her death she spat up a quantity of dark blood; and from this time to her death, on the 26th, respiration was exceedingly slow—it averaged fifteen in the minute. This was remarkable, considering the difficulty of aërating the blood under which the patient laboured. She now became comatose, and remained in this state up to the time of her death.

The *post mortem* examination revealed a typical example of mitral contraction, with pulmonary apoplexy. Both lungs were distinctly emphysematous, and in the apex of each there was an isolated circumscribed mass of very dark effused blood, the surrounding pulmonary tissue being reddened and of a different tint. In these situations the pulmonary tissue did not crepitate on pressure; it was hard and indurated. The base of each lung was of a slate colour, and contained extravasated blood; on the base of the left, where there had been two projections caused by emphysema, there were two masses of blood filling the emphysematous projections; the base of the right lung presented a similar condition. The pericardium contained about six ounces of serum.

The heart, especially on the right side, was greatly distended with dark-coloured blood. It had lost its ordinary figure, and was greater in its transverse than its vertical diameter. The right ventricle was considerably dilated, and its walls were slightly thickened; the right auriculo-ventricular opening was of very considerable size, but the tricuspid valves were healthy, as were also those of the pulmonary artery. The left auricle was large, and its walls considerably thickened; the openings of the pulmonary veins into the left auricle were not dilated. The left ventricle was rather smaller than normal; its walls were not thickened, but the mitral orifice was greatly contracted. The condition of the parts was very remarkable. Both segments of the mitral valve were agglutinated in such a manner as to form a funnel-shaped passage, about one inch and a quarter long, projecting into the left ventricle. The opening at the extremity of this passage would barely admit the point of the little finger. Upon one of the segments of the valve, and upon its auricular aspect, was found a very remarkable fragment of fibrine incorporated with the valve by one of its edges. It was attached to the auricular aspect of one of the segments of the valve, in such a position that it must have been displaced over the orifice by the current of blood passing from the auricle into the ventricle, and formed a sort of valve. There could be no doubt that the vibration of this partially attached fibrine must have caused a murmur synchronous with the passage of the blood into the ventricle, and that it explained fully the presystolic murmur. The aortic valves were perfectly sound, as were likewise the walls of the aorta.

The case presented some features of interest. In the first place, the pulmonary apoplexy, which was typical of its kind, probably occurred a few days before the patient's death—about the same time that she spat up the mass of jelly-like blood. About this time it had been noticed that the respiration became exceedingly slow, not more than fifteen in the minute. The patient's condition was such that any change in posture or the slightest movement caused great uneasiness and distress, so that for the last two or three days of her life he did not examine her chest. Hence he could not say that the effusion of blood into the lung had been detected stethoscopically. This effusion of blood into the lung was not due to an accident of pulmonary hemorrhage, as the effusion was isolated in several situations; and of the two causes which were commonly supposed to give rise to pulmonary apoplexy, namely, the forcible action of the right ventricle and obstruction of the mitral orifice, the latter must have been the cause in operation in the present instance; for, although the right ventricle was hypertrophied, the great dilatation of the auriculo-ventricular orifice must have been sufficient to neutralize the force of the ventricle as directed on the lung. He thought there was sufficient explanation of the pulmonary apoplexy to be found in the obstruction

which the blood must have encountered in its passage from the left auricle into the left ventricle, in consequence of the extreme contraction of the mitral orifice.—*January 28, 1865.*

*Pericarditis.*—DR. BANKS said the pathological specimen which he now presented to the society was taken from the body of a girl, who died on the morning of the 8th instant, in the Hardwicke Hospital. She was twenty years of age; and she stated on admission that she had always been delicate, very subject to attacks upon the chest, and very liable to take cold, and that she also suffered much from catamenial irregularity. About six days before her admission she shivered, and presented (as well as he could ascertain) the ordinary symptoms of commencing fever. She was admitted to hospital on the 31st January. She was found labouring under great respiratory distress, very rapid pulse and great restlessness. On examination of the chest the lower region at each side was found to be dull, and on the right side there was a crepitating râle. She expectorated a brownish fluid, not in the least viscid. The symptoms of respiratory distress went on increasing. She was very intolerant of any motion, but there was no affection of the joints. There was not the least reason to suppose there was anything rheumatic in the case; but she was very feverish, and the respiration rapidly rose until finally, on the day before her death, which occurred on the morning of the 8th, the respiration had attained the number of fifty-six, her pulse then being 140. The dulness increased at each side, and on the left side there was a friction sound. There was then, probably, effusion into the left pleura, and solidification of the right lung. There was dulness anteriorly also, and for the last three days of life there was a *leather-creak* sound over the heart. It was difficult to say how much of the dulness was due to pleural and how much to pericardial effusion. Two or three days before her death there was a patch of redness, about five inches in length and one and a-half inches in breadth, discovered in the vicinity of each elbow-joint, very tender to the touch, and communicating to the fingers a boggy sensation.

On opening the chest there was a considerable quantity of sero-purulent matter discovered, chiefly in the left pleural cavity, but there was some also in the right. The pleura was covered with lymph of a tenacious character; lymph had also been deposited on the right lung, at the base of which there was a small pneumonic abscess, having something of a gangrenous character about it. At the apex of the left lung there was a very small cavity of a similar character, and lined with a membrane. The pericardium contained about eight ounces of pus, creamy, and of a pinkish colour, from the mixture of a small quantity of blood. The surface of the heart was covered with lymph, which was not, however, in the least adherent, for immediately on being touched it separated from

the vascular surface beneath. The pleuro-pneumonia evidently preceded the pericardial inflammation. There was no endocarditis, and the tissue of the heart was perfectly firm, and there was not a trace of lymph on the valves.

Dr. Banks was of opinion that this had been a case of diffuse inflammation—an example of blood-poisoning. The implication of so many parts, the patches of redness on the joints, and the character of the effusion pointed to this conclusion. The sufferings of the patient were extreme. He did not think he had ever seen a patient who seemed to endure such intense agony; and for the last two or three days, so intolerant was she of the slightest disturbance, that it would have been an act of cruelty to institute a very minute examination.—*February 11, 1864.*

*Smallpox Pustules in the Trachea, &c.*—DR. CORRIGAN observed:—The preparation which I exhibit possesses but one point of interest, but that not an unimportant one, namely, as to whether tracheotomy is an operation to which we should have recourse in cases of smallpox where the larynx is so much engaged as to threaten suffocation. The preparation consists of the trachea, larynx, and œsophagus of a boy, twelve years of age, one of nine at present in the Hardwicke Hospital in smallpox, who came from a school at Cabra, on the North Circular-road, where the disease had broken out. The boy had never been vaccinated. The disease ran into the worst form, the confluent, and on the eighth or ninth day threatened the fatal termination which occurred. About the eighth or ninth day, in addition to the secondary fever which then comes on, the boy suffered under a great difficulty of swallowing, so that any portion of fluid taken into the mouth was thrown out with violence. The child seemed to have an absolute terror of swallowing, not unlike that exhibited in cases of hydrophobia. The voice became raucous, and was soon lost. The tongue, as well as the soft palate, was covered with the pustules of smallpox; the pharynx is covered with pustules, which suddenly terminate where it joins the œsophagus, and the line of demarcation is exceedingly well marked; the pustules are thickly spread over the whole of the pharynx, the larynx, and the back of the epiglottis. I will now turn the other side of the preparation, and you will see that the operation of tracheotomy could not be thought of. The larynx is studded with pustules, and a mixture of pustules and lymph; the trachea is in a similar condition down to the bifurcation of the bronchi; so that if any operation were attempted it would open into a diseased surface and tissue, and aggravate the sufferings of the patient without even prolonging life.—*February 11, 1865.*

*Ovarian Tumour.*—MR. HAMILTON exhibited a large ovarian tumour, partly encysted and partly solid, which had been removed from the body



of a woman who appeared to be about thirty years of age. When she came into hospital, nine months ago, her belly was exceedingly distended, and that distension was evidently caused, in a great measure, by fluid. Fluctuation was exceedingly free, and the surface of the abdomen was permeated by large blue veins. She was emaciated to the last degree, and both her lower limbs were œdematous. She suffered considerably from dyspnea, owing, no doubt, to the displacement of the abdominal viscera, which were pressed upwards on the diaphragm. He would have tapped her at once, but there were symptoms which deterred him at the time from doing so, for she evidently laboured under peritoneal inflammation, complaining of great pain in the right side of the abdomen, particularly in the region below the liver, which was exquisitely tender, and till the inflammation was subdued tapping could not be thought of. He waited until he had subdued this inflammation by blistering and other means, and then drew off thirteen quarts of fluid. It was transparent, and of a brownish colour, like sherry and water. This operation was followed by very considerable relief, and at once she began to regain flesh. He supposed the relief of the oppressed organs was followed by increased powers of digestion and circulation; but it was quite astonishing the rapidity with which she put up flesh. The day after he drew off the fluid he was anxious to examine the state of the abdomen, with the view of considering the question of the ultimate removal of the tumour by operation. He found, however, on the left side of the abdomen, an indurated mass, about the size of three fists, uneven, nodulated on the surface, and which conveyed to his feeling the impression that it was a malignant tumour. It had that mixture of fluctuation with solidity and elasticity which, together with the nodulation of the surface, rendered it almost certain that it was of a malignant nature. He, therefore, made up his mind that it was not a case in which an operation for the removal of the tumour could be contemplated. Besides, he could not but believe, from the pain she suffered, on admission to hospital, from peritonitis, and also, previous to her admission, in different parts of the abdomen, that adhesions were very extensive. Both of these reasons, but particularly the presence of a large solid mass within the abdomen, prevented him from entertaining the idea of an operation. However, as his experience of these cases was to a certain degree limited, he was very glad to have the assistance of the opinion of his friend, Dr. McClintock, who, having seen the case, entirely agreed with him as to the inadvisability of any operative proceeding. His colleague, Dr. Gordon, who had operated himself, entertained a similar view. During the nine months that she was in hospital he tapped her eight times. She was always so much relieved by the operation that she was anxious for its repetition, and wished that it should be performed sooner than he thought advisable. After the first operation the nature of the fluid entirely changed; at first it

was transparent, and like sherry and water; afterwards it became opaque, and seemed exactly like thick gruel, and through it were seen shining flat bodies, which, on examination, proved to be cholestrine. When examined by the microscope, numerous pus globules were also seen in the fluid. The woman gradually grew worse and worse, and after the seventh tapping she was so much reduced and emaciated—she suffered from such constant vomiting, and from repeated attacks of diarrhea—and was so feeble, that on two or three occasions he thought she was going to die; and he came to the conclusion that he would not tap her again. However, she suffered so much from the pressure of the tumour that she earnestly asked him to do so, and, very reluctantly, a month before she died he did tap her, and with considerable relief. A week before she died the aperture he had made in tapping her opened of itself, and gave her some temporary relief. On the occasion of the last tapping he felt so much solid tumour at the lower part, and bulging across the place where he had tapped her before, below the umbilicus, that he selected a spot above the umbilicus on the last occasion. She died on the 9th instant. He now exhibited the tumour in the same condition as it was when she died. They would see that it was composed partly of an encysted tumour, containing fluid, and partly of a large mass of solid matter, which he had no doubt was of a malignant nature. On the back of the tumour were to be seen the uterus and the ovaries; the left ovary was tolerably healthy, and the disease seemed to occupy the right. It had formed adhesions in front to the abdominal parietes, so close that the muscular structure had to be cut away in removing the tumour, and it required the greatest possible force to separate them at all; and the adhesion was so close to the liver that Mr. Harman, the resident pupil, who made the *post mortem* examination, was obliged to cut away a portion of the liver with the tumour. There were also adhesions of the upper part of the tumour to some of the intestines. The latter were pushed upwards to the diaphragm, which was thus moved so far up that the superior portion of the heart was on a level with the third rib; it looked like a heart that was compressed, was of an unusually small size, and weighed only five ounces and a-half. The kidneys were found to be healthy, but there were tubercles in the lungs. The solid part of the tumour was formed by a thick-walled cyst, no doubt of a malignant nature, containing a gelatinous fluid of a description familiar to every one who was acquainted with these ovarian tumours. The pathological characters of the ovarian tumour, and the extensive and close adhesions, proved the prudence of not undertaking the operation for its removal.—*February 11, 1865.*

*Ulcer of the Stomach.*—DR. HAYDEN brought under the notice of the Society an example of perforating ulcer of the stomach. The viscera now before them were taken from the body of a man, aged thirty-four years,

reputedly temperate. About two years ago he complained, for the first time, of certain sensations of uneasiness in the stomach, such as acid eructations, and a feeling of peculiar discomfort after taking food. These sensations he experienced about two hours after meals, and they generally ended in an act of vomiting; this act relieved him of the sensations completely. He went on in this state for two years—the uneasiness, however, being converted into absolute pain. Occasionally he was attacked with vomiting, as already mentioned, and on three or four occasions he threw up a quantity of dark grumous matter, manifestly altered blood; he likewise passed a quantity of this matter from his bowels. He was admitted into the Mater Misericordiæ Hospital on the 13th instant. He was exceedingly pale; his pulse was quick, but otherwise normal; his tongue clean. He complained of pain about two hours after taking food, either liquid or solid; he also suffered occasionally from acid eructations; his bowels were constipated, and distended with flatus. There was no tenderness whatever over the region of the stomach; and on the most minute examination there was no physical evidence of disease of that organ to be detected. The pain which he experienced after taking food extended to the lower portion of the dorsal spine; it was not at all of a very aggravated character, very little more than uneasiness. On the morning subsequent to his admission it was found that in the interim he had vomited up a quantity of dark grumous matter, which he said was similar to what he had ejected on former occasions. This was a good example of coffee-grounds vomiting, and there were about three or four ounces of it in the spittoon. Dr. Hayden now made up his mind that he had to deal with one of two things—either some form of latent aneurism, finding entrance into the stomach—or, what was still more probable, gastric ulcer. He treated the man on the latter assumption; and on the following day he was attacked, whilst at dinner, with excruciating pain in the abdomen; he writhed under it, and became very much excited. His colleague, Dr. Cruise, who happened to be in the hospital, saw the man about four o'clock in the day, and prescribed for him; he took a very large dose or two of morphia; and he continued in this state, with very little relief, until nine o'clock, when he died of exhaustion. Although the man was exceedingly anemic, still he was not at all wasted.

On opening the abdomen, a large quantity of dark grumous matter was found diffused through the peritoneum; there was not a trace of inflammatory action; there was no peritonitis. On raising the liver from the surface of the stomach, there was found a very good example of perforation of the latter organ. The aperture was exceedingly well defined, and was larger than the largest goose quill; the edges were smooth and rounded, and the orifice was perfectly circular. The stomach was very much thickened in the neighbourhood of this opening, the edges of which and the portions of the peritoneal surface round it were discoloured from



exuded bile, for the gall bladder had lain directly over the opening. In the neighbourhood of the opening was found a quantity of lymph, lying in flakes on the surface of the stomach. On laying open the stomach, he found an ulcer in the immediate neighbourhood of the pylorus, on the anterior wall; on the internal surface the ulcer was one inch in diameter, and perfectly circular. Immediately behind this, and upon the posterior wall of the stomach, was found another ulcer, much larger in size, being two inches and a half in its long diameter, by an inch and a half in width. The floor of this ulcer was formed by the adherent pancreas, which was held closely attached to the posterior wall of the stomach. Another ulcer, of much smaller size than either of these, was observed in the immediate neighbourhood of the cardiac orifice of the stomach. This had not penetrated the walls of the stomach. It was of the size of a very large pea, its edges well defined; there were no evidences of inflammatory action, and it appeared as if the mucous membrane of the stomach had been punched out. In front of the penetrating ulcer was observed an irregular corrugated patch on the mucous surface, evidently an old cicatrized ulcer.

This case was of interest in one or two particulars:—Firstly, as affording a satisfactory explanation of the absence of epigastric tenderness on pressure; for here we had the right lobe of the liver lying immediately over the ulcer in the stomach, the fundus of the gall bladder lying over the opening; hence any degree of pressure did not give rise to tenderness for the liver lay between the hand of the examiner and the diseased portion of the stomach. Secondly, it appeared to him that the immediate cause of death was not exactly perforation of the stomach, but the detachment of a very frail adhesion between the fundus of the gall bladder and the peritoneal surface of the stomach. On the surface of the gall bladder was a thin layer of lymph, corresponding to the portion of the stomach that it lay in contact with. It was very possible that that lymph, which acted as a bond of cohesion between the gall bladder and the stomach, was of a frail character, owing to the constant transfusion from the gall bladder into it; and when the man took a full meal, this adhesion was broken up, and the contents of the stomach were discharged into the cavity of the abdomen. The appearance of the ulcer showed this; it was well defined, without jagged edges; and the presence of coagulated lymph diffused in the immediate neighbourhood of the opening still further tended to confirm this view. Thirdly, the case was of interest as regards the condition of the patient; he was not emaciated, though the disease had lasted over two years. There was an explanation of this remarkable circumstance, probably, to be found in the fact, that notwithstanding that the pyloric extremity of the stomach was diseased, the remaining portion of the organ was in a healthy state, and the man was therefore capable of absorbing albuminous aliments at all periods since the commencement of his illness.—*February, 18, 1865.*

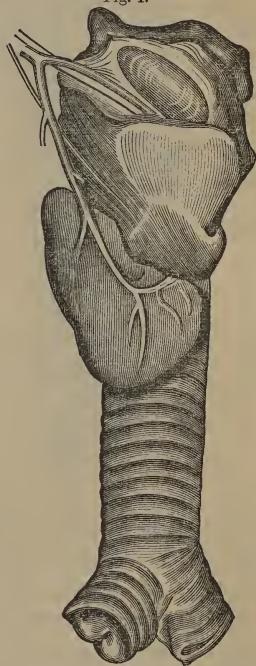


*Vesico-Vaginal Fistula.*—DR. BANON exhibited the edge of a vesico-vaginal fistula, removed by Dr. Kidd at the Coombe Lying-in Hospital. They were all aware that the treatment of vesico-vaginal fistula had within the last few years been very much improved—a new era in the treatment of this formidable and most unpleasant affection had, in fact, been inaugurated during the last ten or twelve years. They were also aware that these fistulæ varied in size; the vesico-vaginal septum was in some instances so extensively injured that the greater portion of the bladder protruded into the vagina; in other instances the fistula was so small as scarcely to admit a small probe, but still large enough to cause all the symptoms of this distressing malady. About two years since he met with one of these small pin-hole fistulæ, as they are termed, and was assisted in the examination of it by Drs. Beatty and Spencer Wells, of London. It was situated very high up in the vagina, which was narrow; the subject was an old woman, aged seventy. Owing to the narrowness of the vagina, and the distance at which the fistula was situated from the external parts of the organ, there would be very great difficulty in paring its edges. Under these circumstances he was put to his wits' end to try to overcome the difficulty, and had invented a little instrument, which was exceedingly simple, and acted very well in assisting the operator in paring the edges of these small fistulæ; it was a steel bent probe, or sound, fixed in a handle; it could be forced into the smallest fistula; projecting backwards there were four elongated points, which, on withdrawing the instrument, after having introduced it freely through the fistula, would grapple or catch its edges on every side. Previous to introducing it the object would be much facilitated by passing a sound into the bladder, and raising the vesico-vaginal membrane towards the operator. In this way the operator could pass the instrument through the small fistula; and, holding the instrument in the left hand, and making the parts tense, it became very easy to divide them, passing the knife so as to cut out the instrument, including of course the whole edge of the fistulous opening; this is one of the principal advantages to be derived from its use. The parts as removed are here seen, still attached to the instrument, and the elliptical shape was selected by Dr. Kidd as that most convenient for subsequent coaptation.

Dr. Banon has recently published a series of ten or eleven successful cases of operation on vesico-vaginal fistulæ, three or four of which were of the small or pin-hole variety, in which he had used the instrument referred to, and now exhibited to the society. It had also been successfully used on two occasions by Dr. Kidd, and also by Dr. Cronin. His object in bringing the subject before the society was, partly to show this frequent form of fistula, and also an instrument which he and others who have used it, found of advantage in the surgical treatment of this affection.—*February, 18, 1865.*

*Malformation of the Larynx.*<sup>a</sup>—DR. BENNETT exhibited a human larynx in which there existed fully developed laryngeal pouches, such as are found in the class *Quadrupana*. He obtained the specimen, accidentally, in the dissecting-room of Trinity College, in the body of a male subject, about twenty-four years of age. While dissecting over the thyro-hyoid membrane, in order to demonstrate the presence of the bursa which usually occurs over the projecting portion of the thyroid cartilage, his attention was arrested by a cystic structure, which was opened accidentally, on one side; a probe, passed downwards through the opening, entered readily into the larynx. Thinking, at first, that this cavity and passage might be the result of disease, he at once examined the corresponding region on the opposite side, and found a similar sac there. This fact, as well as the healthy appearance of the structures composing the

Fig. 1.



sacs, suggested that the case was one of abnormal development of the laryngeal pouches. In order to make a careful examination of the larynx, he removed it, with the hyoid bone and thyroid body attached to it. On the right side (Fig. 1.), the thyro-hyoid muscle being removed, the pouch is seen in relation to the surrounding parts; it occupies nearly all the space which is covered by the thin lateral portion of the thyro-hyoid membrane. Appearing above the border of the thyroid cartilage, immediately external to the lower portion of the mass of fat erroneously called the epiglottidean gland, the sac extends, upwards and outwards, almost as far as the lateral thyro-hyoid ligament; its lower border rests on the superior laryngeal vessels and nerve as they pass into the larynx; superiorly the sac is in contact with the epiglottidean gland and the under surface of the great horn of the hyoid bone. The membrane forming the sac is thin, very loosely connected to the parts around it, and is studded with numerous small glands. On the left side (Fig. 2.) the ala of the thyroid cartilage has been removed, and its section is seen in front; the thyroid gland and crico-thyroid muscle are also removed on this side. By this dissection the sac is exposed down to the point at which it opens into the larynx. Immediately in contact with the lower part are the fibres of the thyro-epiglottidean muscle, and below these are seen the thyro-arytenoid and crico-arytenoid muscles; the fibres of the first-named

<sup>a</sup> The preparation is preserved in the Anatomical Museum of Trinity College.

muscle pass behind the sac as they go to be attached to the epiglottis. On examining the interior of the larynx (Fig. 3.) the probe passed

Fig. 2.

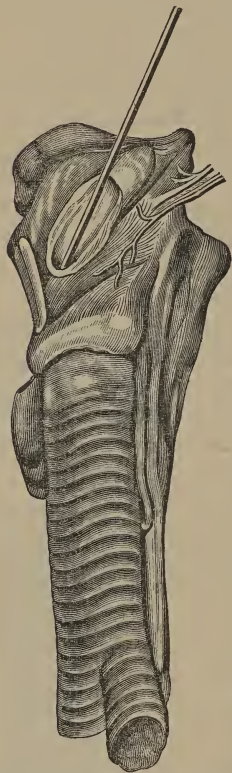


Fig. 3.



through the sac from without appears beneath the anterior part of the false vocal chords, and enters the ventricle of the larynx by an opening of oval shape, about three-fifths of an inch in length. It will be seen that the position of this opening is exactly the same as that of the little pouch called *sacculus laryngis*, which normally opens off the highest point of the ventricle of the larynx, and which has been described and figured by Morgagni and Hilton.

The fibres of the inferior aryteno-epiglottidean muscle pass across the laryngeal surface of the lower end of the sac, and can be exposed by raising the mucous membrane. In their position, shape, and relations these pouches resemble exactly the lateral laryngeal pouches of *Quadrumanus*; and their development in this case shows that, whatever function we may assign to those organs, we must regard the *sacculus laryngis* of Hilton as the rudiment of them in man. Even a single case, such as this,



must lead us to modify the statement of Dr. Carpenter as to the visceral peculiarities of man, especially as the sacculus laryngis, though variable, is very constant in its presence.

Dr. Carpenter says as follows, in the chapter in which he treats of the distinctive peculiarities of man (*Principles of Human Physiology*, p. 10):—"The visceral apparatus of man presents very characteristic peculiarities, by which it can be distinguished from that of the higher *Quadrumana*; among the most remarkable is the absence of the laryngeal pouches, which exist even in the chimpanzee and ourang outang as dilatations of the laryngeal ventricles." We cannot admit this statement as regards the lateral pouches, and not even absolutely as regards the central pouch of *Quadrumana*. There is not, indeed, any instance on record of a central pouch in man communicating with the larynx, but we have the representative of the pouch itself in the bursa which occurs in man over the point of the pomum Adami; this bursa has all the anatomical relations of the pouch, except its opening through the stalk of the epiglottis.—*February 25, 1865.*

*Cirrhosis of the Kidneys.*—DR. BANKS said, the morbid specimen which he now presented to the society was taken from the body of a woman, who died on the 22nd inst., in the Whitworth Hospital. She was aged twenty-seven, and was received into the hospital on the 27th of last October. She stated that until three or four days before her admission she was in the enjoyment of perfect health. She had a severe wetting, got chilled, and shortly afterwards suffered from pain in the back, with frequent passing of water, and she soon perceived that there was some swelling in her face. On her admission into hospital, on the third or fourth day after her first complaining, it was found that she was labouring under acute renal dropsy. There was anasarca of the whole body, and some effusion also into the peritoneum. The urine was a little above the normal density, of high colour, and abounding in albumen. Under treatment the dropsical symptoms completely disappeared; and at the expiration of about six weeks, thinking herself quite well, but contrary to advice, she left the hospital. She appeared to be well, but the urine still contained some albumen. She went on for a considerable time, according to her own account on re-admission, in the possession of perfect health; but again she was exposed to cold, and there was a recurrence of the same train of symptoms. She was received into hospital for the second time on the 29th of January.

She was then anemic, and universally dropsical; the cellular tissue all over the body was the seat of effusion. She had also bronchitis, and the urine was then found to be totally different from the conditions that it presented on her first admission: it was now of low density (1011), much less albuminous, and on microscopic examination it presented the



characteristic appearances which belong to chronic renal disease. From the time of her second admission no impression was made by treatment on the dropsical symptoms; and on the 14th of last month she, for the first time, was found to be extremely drowsy; in fact, she was asleep, and snoring loudly, on the occasion of the daily visit, and the patients said she was asleep the greater part of the day before. It was quite evident that uremic intoxication was impending. The second night after this she was attacked with severe convulsions; the right side of the body being more affected than the left. After the convulsions ceased, the patient passed almost into a state of coma, but she could be roused by speaking sharply to her. On the next day she was in pretty much the same state as before the attack, still drowsy, but intelligent, and able to answer questions. A few nights afterwards she had another attack of convulsions, and altogether she had five or six of these attacks, and in one of the fits she bit her tongue slightly. On the occasion of the last fit, which occurred on the 22nd of February, the last day of her life, Mr. Little, the resident pupil, observed her in the fit, which continued ten minutes, and she passed from it into coma. The right side was violently convulsed, much more than the left, which was also affected.

On examination after death, it was found that the brain was very remarkably anemic. There was an inconsiderable amount of serum in the ventricles, but there was a good deal under the arachnoid membrane. The kidneys afforded a beautiful example of the small contracted cirrhus kidney, hard and lobular; the right kidney weighed but two ounces and a quarter, and the left weighed barely two ounces. The capsule was firmly adherent to the tissue of the kidney underneath. The liver was also small, and doubtless the process of cirrhosis had commenced in it. The amount of fluid in the peritoneum was much greater than could be accounted for by looking upon it as part of the general dropsy. The bronchial tubes exhibited the usual appearances of inflammation. The heart was enlarged, particularly the left ventricle; and the valves were thickened at the edges, and puckered. One point of interest to which Dr. Banks wished to direct the attention of the society was, the rapidity with which the disease ran its course. They were familiar with cases which, commencing like the present, lasted for years. Here, however, was a case which could not be traced back further than the 29th of September. He believed that the small cirrhus kidney in this case was the result of acute disease; and, if so, it was a remarkable exception to the general rule, for assuredly the process of cirrhosis is almost always chronic.—*February 25, 1865.*

*Malignant Tumour.*—MR. TUFNELL exhibited a tumour which he had removed a few days previously from the outer border of the thigh of a man, fifty-four years of age. He received, in August, 1863, a kick from a

horse, which was followed by ecchymosis at the time. Shortly afterwards he observed a small warty growth at this place about the size of a stud button; but whether this was a coincidence, or a consequence, it was impossible to say. This warty growth gradually increased into a tumour, which at first was pedunculated, but subsequently changed considerably; and when the man came under Mr. Tufnell's observation the base of the tumour was broad—fully seven inches long, and three or four wide, exceedingly dense, and firmly attached at the base. It had three nodules on the surface, each of the diameter of the top of the thumb; one of which was perfectly livid, and on puncturing yielded its contents—pure blood.

Mr. Tufnell removed the tumour, with a portion of the muscular structure, which was necessary, so firmly was it attached. Upon turning the tumour up, and examining its base, it exhibited three other nodules; so that, whilst three came forward anteriorly and externally, the same thing took place posteriorly at the base, and down into the structure of the thigh. The question then arose as to where the disease originated—whether there was a cutaneous disease generated at the surface, and a deep-seated malignant disease also generated as the result of the injury to the deeper seated structures below. He thought they could hardly doubt, looking at the section of the tumour, that it was malignant.

Dr. Barker, Curator of the Museum of the College of Surgeons, had examined it under the microscope, and the cells presented all the characters of malignant disease, and to the naked eye it gave that appearance. It resembled very much a case of malignant disease of the testicle described by Sir Astley Cooper; the glands in the groin were slightly enlarged.

Mr. Tufnell remarked, in conclusion, that he would be able to trace the subsequent history of this man, and would make it his duty to report it to the society hereafter.—*February 25, 1865.*

*Caries of the Tarsal Bones.*—MR. CROLY said, that he had the honour of laying before the society, at its first meeting, a foot affected with scrofulous caries, for which he had performed Syme's amputation at the ankle-joint, in the City of Dublin Hospital. He had also on that occasion called their attention to a section of the tibia, showing that the disease extended into that bone; requiring amputation higher up. He now had to lay before them a very interesting specimen of caries of the bones of the foot, affecting the cuneiform, cuboid, and scaphoid bones. The disease commenced at the junction of the metatarsal bone of the great toe with the internal cuneiform. The patient was of a scrofulous habit. When admitted into the City of Dublin Hospital under his care, in November last, he intended to remove the metatarsal bone of the great toe with the internal cuneiform. On the day fixed for the operation, however, the

man was attacked with a severe rigor, followed by erysipelas, which was confined to the foot. He made a free incision on the dorsum of the foot, to relieve the tension; and subsequently an abscess formed underneath, where the disease commenced, requiring another free incision. The man was treated by antiscrofulous medicines, and was given good, nutritive diet. He left the hospital; but soon returned, very much exhausted, and requested him to operate; but his health at that time not being sufficiently good, he was obliged again to postpone it. On last Tuesday he performed the operation known as Chopart's, cutting from the spur on the scaphoid bone on the inside to the cuboid bone on the outside, and making the flap from the sole of the foot. It was very interesting to observe that the disease in the cuboid bone had gone as far as the cartilage, and in the scaphoid bone it had invaded a portion of the cartilage. There was no disease in the head of the astragalus.—*February, 25, 1865.*

*Echymosis of the Stomach.*—DR. BENNETT exhibited a specimen of ecchymosis of the mucous membrane of the stomach. He was unable to give any history of the case during life, as the specimen was taken from the body of a middle-aged man, that had been brought into the anatomical theatre of Trinity College. It was a good deal wasted, but presented no external signs of purpura. One leg had been amputated below the knee long before the death of the individual. Upon opening the stomach, the contents were found to be of the ordinary character, and free from any trace of blood, either pure or altered by gastric action. There was no trace of blood in any part of the intestinal canal. The mucous membrane of the stomach presented a very remarkable appearance; its entire surface was marked with circular spots of ecchymosis, ranging in size from that of a fourpenny piece to mere specks, resembling the cutaneous eruption of purpura, except that their colour was coal black, and their surface convex. The blood constituting these spots was deposited immediately under the epithelium of the mucous membrane, and both the epithelium and it could be removed by the slightest scraping; in the intervals between the spots the membrane looked natural, and was free from any sign of congestion. The appearance of the eruption at first sight was such as one might imagine to have been produced by sprinkling the membrane with concentrated oil of vitriol. Cruveilhier has represented a similar appearance, accidentally discovered in the body of an individual who had died of some chronic malady unconnected with disease of the stomach. The only difference between the cases was, that in Cruveilhier's there was blood found in the intestines, while in the present one the eruption on the membrane was more regular, and the spots much more numerous. The condition of the viscera of the abdomen did not account for this escape of blood. The liver seemed quite healthy, there being nothing abnormal about it. The gall bladder contained several of that

rarer form of gall stone called by Budd "hepatic gall stone." The only information which the examination of other organs gave of the cause of the condition of the stomach was derived from the state of the arterial system, as might be seen by the portion of the aorta exhibited, the coats of which were affected with atheromatous disease in every part; and the removal of the deposit had in two places led to the formation of small dissecting aneurisms. All the arteries of the body were more or less diseased, and probably the minute vessels of the stomach were similarly affected.—*March 4, 1865.*

*Disease of the Aortic and Mitral Orifices.*—DR. HAYDEN exhibited an example of disease of the heart, and of peritonitis consequent upon the operation of paracentesis abdominis. The patient was a female, aged fifty-one, who had travelled in many parts of the world in the capacity of governess, and had enjoyed excellent health up to the 8th of August last. On that day she observed, for the first time, on attempting to put on her boots, that her feet were swollen. Within a few weeks subsequently there was considerable effusion into the peritoneum; and for the relief of the distress of breathing consequent upon this, the operation of paracentesis was performed on two occasions. On the first occasion four gallons of fluid were removed, and on the second about half that quantity. She was admitted into hospital on the 23rd of February. Her face was then livid; the conjunctivæ were slightly jaundiced; the pulse 96; the breathing was greatly embarrassed, and weak, but regular. There was much effusion into the peritoneum. On placing my hand upon the heart, I detected a rather feeble impulse; and on applying the stethoscope I discovered a murmur with the first sound below and a little to the outside of the left nipple. It was still more distinctly audible over the lower part of the sternum, and thence it was traceable upwards along the course of the ascending portion of the arch of the aorta to the junction of the third right costal cartilage with the sternum, and no further. The second sound was normal; respiration was natural over the anterior part of the chest. Over the base of the left lung and posteriorly there was dulness, and over both places, behind, loud crepitant râles were audible. She besought me earnestly to perform the operation of tapping to relieve the difficulty she felt in respiration, and I accordingly did so on the evening of the 23rd February, and drew off two and a half gallons of amber-coloured serum. The operation was followed by great relief to the breathing. I was struck with the remarkable appearance presented by the liver. When the abdominal walls collapsed, after the removal of the fluid, its inferior left margin stood out prominently, exactly parallel with the right costal cartilages, and three or four inches below them. An examination of the left side showed there was no enlargement of the spleen.

On the day after the operation (24th February) the pulse had risen to



132, and was weak. The liver had subsided, and now occupied its ordinary position. The diagnosis which I formed after repeated examinations was that of regurgitant disease of the mitral orifice, and obstructive disease of some form at the aortic orifice, together with engorgement of the liver consequent upon the disease of the heart. The patient, two or three days subsequently, complained, for the first time, of tenderness of the abdomen. I had little doubt on my mind, taking this symptom in conjunction with the others, and also from the expression of the face, that peritonitis had been set up in consequence of the operation. The patient died on the 28th February.

On opening the abdomen, a large quantity of opaque milky-looking fluid, at least one and a half gallon, was discovered, with flakes of lymph floating in it. The entire peritoneal surface presents *the peculiar* appearance characteristic of acute peritonitis. The condition of the liver was such as to show that the patient had been the victim of tight lacing; it presented a transverse groove, corresponding to the right costal cartilages; the pressure exercised in this direction had elongated it, and at the same time diminished its width from right to left. The falciform ligament had become greatly thickened, and lay for a distance of an inch and a half upon the anterior surface of the left lobe, to which it adhered. The liver was also somewhat larger than usual, but in other respects was healthy.

The pericardium contained four ounces of fluid; and there was a considerable deposit of fat upon the anterior surface of the heart, but it did not invade the muscular structure. The right ventricle was not enlarged, but its walls were thickened; occupying the right auricle, and thence descending into the ventricle was a flake of fibrin, which, by fixing the anterior end of the right segment of the tricuspid valve to the corresponding wall of the ventricle, had rendered it incompetent. The left ventricle was decidedly thicker in its walls than it should be; the cavity was rather contracted than dilated. The anterior and right segment of the mitral valve was greatly thickened, and rather rough on the surface, while the posterior and left segment was quite unaffected. On passing my finger into the aortic orifice, I experienced a grating sensation; and on examining into the cause, I found a considerable deposit of calcareous matter on that portion of the internal surface of the aorta corresponding with the margins of the everted valves. Above this the aortic wall itself was in a state of incipient atheromatous degeneration; the aortic valves were healthy. The lungs were engorged, especially their inferior portions.

The interest which this case possesses, if any, is chiefly connected with the state of the aortic orifice, but in some measure also with the unusual position of the liver. We have here an example of disease of the walls of the aorta (not the valves), giving rise to the phenomena commonly interpreted as evidence of disease of the valves themselves, yet the valves were healthy. The systolic murmur was traceable upwards to a certain

distance, the second sound being healthy; there was at the same time disease of the mitral orifice; this, of course, gave rise to the murmur which was heard in the region of the apex of the heart. The unusual position occupied by the liver, after the operation, I was disposed at the time to attribute to adhesion of its anterior surface to the abdominal wall, as a consequence of the previous tapping; as now shown, it was due to shortening and adhesion of the falciform ligament, as already mentioned.—*March 4, 1865.*

*Croup.*—DR. DUNCAN exhibited a preparation illustrative of a case of croup occurring in an infant thirteen months old. The child was brought to the dispensary of the hospital on the previous morning, and was seen by his colleague, Dr. Walsh. It was in perfect health on Thursday morning, at twelve o'clock; it was then taken out into the open air by its grandmother, and about half-past one o'clock was seized with difficulty of breathing. Yesterday morning it was brought into the hospital almost in a state of asphyxia, with intense dyspnea, and occasional but distinct croupy breathing. It was placed under his (Dr. Duncan's) care in the hospital, where he saw it at half-past eleven o'clock. On examining the chest, he arrived at the conclusion that lobular pneumonia existed at the base of the right lung. The grounds on which he rested his diagnosis were these—not simply that it is the most common form of pneumonia occurring in infants, but because there was not that marked dulness on percussion which occurs when there is extensive solidification of the lung; nor was there any crepitus; there was an entire absence of bronchial r  le; there was bronchial respiration, and the wooden sound on percussion in that part of the lung. Convulsions set in on Friday evening, and the child died that night.

On examining the body, a false membrane was found lining the entire of the trachea down to its bifurcation—not a particle of it appeared above the rima glottidis: the membrane was tenacious, and well formed. On examining the several bronchial tubes, there did not appear to be much evidence of bronchitis, but there was lobular pneumonia in that portion of the lung which gave indications of it during life.

The case was interesting in some points of view. First, as showing the extreme rapidity with which a false membrane of this kind can be formed; it showed, also, the utter inutility of any surgical interference. The case was remarkable in this respect, that the croupy sound of the respiration was only heard occasionally—a point which might lead to the suspicion that the case was one of spasmodic croup; the history, however, showed clearly that this was not so. Then there was no evidence of its being of a diphtheritic character; for the pulse was not the weak compressible one which was met with in patients suffering from diphtheria. The other lung was healthy in the greater part of its extent, but presented in one

part appearances of an unresolved pneumonia, from an attack of which the child had suffered six months previously, the lung being carnified. Round the solidified portion there was an emphysematous condition of the pulmonary tissue.—*March 11, 1865.*

*Pneumothorax.*—DR. CORRIGAN brought under the notice of the society a case of pneumothorax, occurring at a much earlier period than usual in the progress of phthisis. The patient was a policeman, forty years of age, who was on duty up to the 23rd of February; and the history he gave of his case, when he entered hospital, was, that he had been in perfect health until November last, when, having heated himself in arresting a man, and having been on night duty for some hours afterwards he caught cold; he coughed and expectorated, though not in large quantities, and continued at his duty up to the 23rd of February. He was then on duty in the Bank, when he was seized with a sudden fit of severe coughing, which continued twenty minutes, and which he described as being of an extremely spasmodic nature. In this fit of coughing he was seized with pain and with shortness of breathing, and his distress became so great that he was obliged to retire from the Bank. The dyspnea continued, and he was admitted to hospital on the 27th of February. We examined him on that day. As he sat up in bed he presented the appearance of a man who had received a great shock. His face was pallid, his lips livid, but not congested; there was no evidence of venous congestion about him; his respiration was very rapid. On examining him, the following evidences of the existence of pneumothorax were discovered:—The right side of the chest was resonant on percussion; the left was in a normal state; the heart was not displaced. On using succussion, we could not perceive that peculiar splashing sound that indicates an admixture of fluid and air in the cavity of the pleura; but on applying the stethoscope over the third and fifth ribs, loud amphoric resonance was observed. On applying it over the right scapular region, the amphoric resonance was extremely loud. There was, then, no doubt as to the nature of the disease; it was evident that a fistulous communication had taken place between one of the bronchial tubes (probably through a tubercular excavation) into the cavity of the pleura.

From the shock he had sustained he never rallied; his pulse was extremely weak, and his respiration rapid, but there was no congestion anywhere, or evidence of pressure obstructing the return of the blood through the veins. He was relieved from these symptoms, and remained in hospital from the 27th of February to the 8th of March, when he became exceedingly weak, lying in bed with a pallid countenance, and scarcely perceptible pulse; he sank gradually, and died that night.

On making a *post mortem* examination, the left lung was found congested and tubercles were deposited here and there. Upon the right side



the intercostal spaces were bulged out, and the chest tympanitic; and on passing a tube through the parietes, the air rushed out with such force as to blow out a candle. On examining the lung it was found carnified; it resembled a piece of muscle, and sank in water. The pleura was covered with lymph of an unhealthy character. On exposing the right lung, and following the bronchial tube down, we came to a cavity of rather small size; there were tubercles scattered about it, but the cavity itself had no morbid deposit around it. It would probably have got well, and become a mere secreting cavity lined with a membrane, and life might have been preserved for many years, but for the attack of pneumothorax and pleuritis. The fistulous opening by which the air escaped admitted of the passage of a small straw. The history of the case, and the appearances presented on dissection, all correspond so far. The only point of great importance in the case is the fact that the rupture took place so very early in the course of phthisis.

There is, however, another point deserving of attention:—As already stated, the man had been relieved from the distress of breathing; but on the day before he died, began to grow very weak, and the pulse gradually became so feeble that it gave no impulse to the hand, and he sank and died from excessive debility of the circulating system. Dr. Corrigan said he had seen the same thing occur where a patient seemed to be recovering from fever. At a time when he was deemed convalescent, he began to grow gradually weak; the pulse failed, until at last it ceased to be felt; and on examination after death, there was found what existed in the present case also, and what may be called embolism of the heart; that is, the formation of a fibrinous deposit in the heart. It was, of course, of recent formation, and Dr. Corrigan believed that it was formed during life. It is probable that in such a case as this the heart not acting fully each time to expel the blood, a portion in the centre coagulates and forms the polypus, which then rapidly increases in size, and is probably the cause of death by obstructing the circulation. That such polypus may be formed during life he had no doubt, for he had sometimes seen the portion of polypus which lay in the aortic opening constricted and marked by the action of the valves, which could have occurred only during life.

Another reason in favour of this supposition is, that where polypi exist in the right ventricle, a portion of the black blood is found combined with them; whereas in this case it has been almost completely washed away.—*March 11, 1865.*

*Tumour of the Cranium.*—DR. EDWARD HAMILTON said, the preparation which he now exhibited to the society was taken from the body of a female, who was for several months under his observation in Steevens' Hospital. She was admitted complaining of a tumour in the upper portion



of the frontal region; it was soft, pulsating, and could apparently be diminished by pressure; it presented, however, no bruit; but vessels of tolerably large size could be found coursing over its surface; she did not complain of pain.

The history she gave of the case was, that she had suffered some slight pain or uneasiness while carrying a heavy basket on her head, some months before her admission into hospital, and since then it had increased considerably. Her general health appeared tolerably good, and she did not present any of the appearances of malignant cachexia.

It now became a matter of very anxious investigation as to whether or not this tumour communicated with the cavity of the cranium, but there was no evidence whatever to lead to the conclusion that such was the case. The tumour presented no respiratory movement; it presented no impulse on coughing; no cerebral disturbance was produced by forcible and direct pressure on the tumour; and the patient did not suffer from vertigo or paralysis. Various methods of treatment were adopted. The vessels leading to the tumour were, some tied, and some compressed. Compression was exerted over the tumour, injection by perchloride of iron was used, and a subcutaneous ligature passed round the base of the tumour, in order to strangulate it. The morbid growth went on increasing in size, until at last the integument ulcerated, presenting a bleeding surface, from which copious hemorrhages occurred at each dressing, and on more than one occasion threatened to cause the patient's death. At last she sank from the effects of these repeated attacks of hemorrhage.

On making a *post mortem* examination, there was found on the external surface a large fungous mass, which on section presented a surface very much resembling brain, with spots of coagulated blood in various parts of it. A considerable portion of the tumour was outside the cavity of the cranium. They had then a constricted portion of it passing out through an irregular opening in the frontal bone, where it joined the parietal bone. The portion of the tumour within the cavity was still more like cerebral substance. The dura mater was very little changed, the external surface merely presenting a few Pachionian bodies upon it. The surface of the right hemisphere of the brain presented a distinct depression, corresponding to the seat of the tumour; yet the woman never at any time complained of the slightest symptoms of cerebral disturbance, barely even complaining of headache. When she was some time under observation she drew attention to a tumour which occupied the sternal end of the clavicle; it was round, softened, presenting to the eye distinct evidence of pulsation, and to the ear a distinct *bruit de souffle*.

The question might arise as to whether this tumour originated from the dura mater, or from the diploë, and made its way both internally and externally. He thought the appearance of the disease in the second situation, so closely corresponding to the appearance on the head, would

show that the disease was in the first instance of osseous origin.—*March 11, 1865.*

*Disease of the Mitral Valve; Pulmonary Apoplexy.*—DR. DUNCAN brought under the notice of the society a case of pulmonary apoplexy which presented some points of interest. The subject was a female, aged forty-six, who was admitted to the Adelaide Hospital on the 27th of January, under the care of Dr. Head, with the signs and symptoms of pneumonia; she had cough, dyspnea, the peculiar aspect, the rusty expectoration, with dulness on percussion, and distinct crepitus; but the pneumonia was local, being confined to a small portion of the upper part of the left lung. The case was intrusted to Dr. Duncan's care about the 4th of February, and shortly after this he discovered, for the first time, a bruit with the first sound towards the apex of the heart. The bruit was soft and short, but towards the close a little rough; there was a distinct interval of rest in the heart's action. It was stated to him that no bruit had been previously observed; from this he was led to infer that he had recent endocarditis to contend with; this was a mistake, the lesion proving to be disease of the mitral valve; but, though the narrowing of the aperture was considerable, there was a certain amount of regurgitation permitted. It was an old lesion, instead of one that had come on during her last illness. The woman stated that for seven years before this she had been subject to palpitation, but there was no other evidence of previous heart disease. At the time of her admission into the hospital, she laboured under œdema of the lower extremities. The case did not progress satisfactorily; although the pneumonia diminished in intensity, it was not subdued; the dulness on percussion became less, but was not removed, and crepitus remained through parts of the chest. The pneumonia had extended to the lower part of the left lung, and she died on the previous Saturday. About a week before her death she had hemoptysis; the blood was of a bright florid colour, and intimately mixed with expectorations of a viscid character, and contained minute bubbles of air through every part of it.

On examining the heart, he found the left auricle enormously enlarged, with its walls greatly thickened; and the left ventricle was small in comparison to the other portions of the heart. He should have mentioned that there was no bruit whatever at the base of the heart. There was a narrowing of the left auriculo-ventricular opening, with some amount of roughness on the auricular aspect of the valve; but yet this opening, small as it was, was patulous, so as to admit of a small amount of regurgitation from the ventricle into the auricle. The right ventricle was considerably enlarged in capacity, and hypertrophied in structure, and there was a considerable deposit of fat on its surface.

The interesting points of this case were—first, the fact that we have

a pulmonary apoplexy occurring in an attack of acute pneumonia, which he believed to be unusual; and the second was, the character of the hemoptysis. The blood was of a bright florid colour, not copious, but continuous for some days; whereas generally, as far as his observation extended, in hemoptysis in cases of pulmonary apoplexy the blood was of a dark colour. The older pathologists attributed this lesion to hypertrophy of the right ventricle—a condition which, he conceived, was always consecutive to some other lesion, as in the instance under consideration. It was true that there was well-marked hypertrophy present in this case; but he was persuaded that the obstruction to the passage of the blood from the lung to the left ventricle had a great deal more to say to the production of an apoplectic condition of the lung than the hypertrophy of the right ventricle. The narrowing of the auriculo-ventricular opening did not necessarily lead to pulmonary apoplexy, as appeared from another case, a drawing of which Dr. Duncan exhibited, in which this lesion existed in an equal degree to that upon the table, and yet there was no pulmonary apoplexy. In a second instance, free regurgitation through the mitral orifice, producing pulmonary engorgement, was accompanied by apoplectic results, but in an opposite way, showing that it was the congestion of the lung rather than the augmented force of the right ventricle propelling the blood that was the cause of the lesion. The existence of pulmonary apoplexy was not suspected in this case. The only thing that could have suggested the idea was the hemoptysis. But the character of the hemorrhage did not correspond with what has been usually observed in pulmonary apoplexy, while it closely resembled that sometimes met with in pneumonia. Coupling this fact with the complete absence of hemoptysis occasionally noticed in pulmonary apoplexy, Dr. Duncan believed that the hemorrhage in this case was connected with the pneumonia rather than with the apoplexy. There was nothing in the character of the bruit which had been heard at the apex of the heart inconsistent with the notion of recent endocarditis, although the *post mortem* examination showed that it was due to a lesion of old standing—a point which could only have been cleared up by a more accurate history of the earlier course of the case.

Looking at the specimen one might be led to imagine from the small size of the mitral orifice that regurgitation was impossible, and that the bruit which was heard during life must have accompanied the second sound, and not the first. But, notwithstanding the narrowing, regurgitation was evidently possible from the imperfect manner in which the opening was closed when the lips were pressed together, and the small quantity of blood which would be forced back during the ventricular systole accurately corresponded with the short murmur which replaced the first sound. Dr. Duncan does not believe that a murmur is commonly produced by obstructive disease of the mitral orifice, because usually the



current of blood is not passed through with sufficient rapidity and force to generate sound ; but in this instance he thought such a thing was possible, in consequence of the hypertrophied condition of the left auricle, and the roughness of the deposit on the auricular aspect of the valve, which led him to believe that part of the morbid sound of a rougher character occurring in the middle of the murmur corresponded to the forcible passage of the blood from the auricle in the ventricle.—*March 25, 1865.*

*Aneurism of the Aorta.*—DR. BANKS said, the man from whose body the specimens that he presented to the society were taken came under his notice on the 16th of march, 1864. He had been for a short time in Sir Patrick Dun's Hospital, under the care of Dr. Kennedy, and had also been in Mercer's Hospital, under the care of Dr. Moore, who, in a paper on the diagnosis and treatment of thoracic aneurism in the *Quarterly Journal* of May, 1864, had briefly recorded the condition of this man when he first came under his notice. He was then forty-four years of age, and was in his forty-sixth year when he died. When Dr. Moore saw him "he was suffering from neuralgic pains over the top of the sternum, and from a troublesome cough ; and also from dysphagia, which has disappeared. He complained of tightness of the skin of the left half of the face, of tingling sensations of the same half, and of intense heat of both ears at times. There was ptosis of the left eyelid, and drooping of the left angle of the mouth. The left pupil was more contracted than the right, and a herpetic eruption covered the left half of the upper lip and chin, and other patches were present over the top of the chest and shoulders. The superficial veins over the upper part of the chest were remarkable ; and a prominent tumour extended from the right clavicular articulation across the sternum for more than an inch under the left clavicle, over which tumour a second centre of pulsation could be felt. The left radial pulse was indistinct, and the respiration was especially feeble over the left lung. In the month of May last he first had an attack of epistaxis, which gave him relief ; and in October and November it returned again. At the end of this month the semi-ptosis of the left lid, deformity of the left angle of the mouth, herpetic eruption, ringing tracheal cough, partial aphonia, and visible tumour still were present, and the epistaxis had returned ; but the contraction of the pupil, the tightness of the muscles of the face, and the heat of the ears had disappeared, at least to a great extent."

On the 19th of January last I examined this patient, when the following changes had taken place :—There was no perceptible difference in the size of either pupil ; the tightness of the muscles of the left half of the face had almost disappeared, as had also the heat of the ears ; the tumour was not so prominent ; the greatest prominence still existed over the junction of the first and second rib with the sternum, on the right side ; but the pulsation over the tumour was still very decided. The radial



pulses were nearly equable, nor was the cough so distressing; the dysphagia had not returned. These remissions have not occurred for the first time in this case, as the patient tells me that in Autumn, 1861, while in Sir Patrick Dun's Hospital, under Dr. Henry Kennedy, he suffered from difficulty of swallowing and respiration; and that these distressing symptoms were then relieved by the appearance of a large tumour, soft and pulsating, which suddenly appeared at the top of the chest, to the left side. At this time he lost his voice, which was restored to him gradually as the external tumour lessened, which it did in a month; the greatest prominence remaining on the right side.

Four days before this man's admission into the Whitworth Hospital he was seized with severe headache and giddiness, and his articulation became so imperfect that he could scarcely be understood. When he came into the hospital there was still some difficulty of articulation, and his mental powers were evidently very imperfect; his memory was quite gone.

On being asked his age, he stated that he was twenty-four, his real age being forty-six. There was some slight paralysis of the right side of the body, and also of the right side of the face. He was, however, able to walk into the hospital; but on the second or third day after his admission he completely lost all power of motion of the right side, and there was also a manifest diminution of sensibility. On examining his chest, a tumour was found, small in size, but of extreme hardness; it was prominent, and occupied the upper part of the sternum, and extended to the right sterno-clavicular articulation. There was a very slight impulse, and a double sound was audible; there was no bruit, but two sounds identical with the ordinary sounds of the heart were perceived. He complained of great pain in the tumour, shooting down the right arm. This was the most marked symptom present. There was not the slightest difference between the radial pulses, nor between the pupils, which were examined repeatedly at this period with much care; there was some herpetic eruption on the chin, and also on the chest, which afterwards quite disappeared. From the 16th of March until the 23rd of May, when he left the hospital, so far as the tumour was concerned there was no change, but the paralytic symptoms gradually disappeared, and he then recovered the power of motion of the right side, and his memory returned. In fact, he seemed completely, so far as the paralytic symptoms were concerned, to have recovered; the only trace of paralysis that remained was of the face, the mouth being drawn, particularly when he smiled, to the left side.

A fortnight after he left hospital, on the return of severe pain in the chest, and a distressing cough, he was again admitted into Sir Patrick Dun's Hospital, where he remained from the 23rd of May to the 5th of September, without undergoing any remarkable change; and was

transferred, on the 5th of September, to the Whitworth Hospital, where he continued until his death. About last November the cough became more severe and paroxysmal in character. A change took place in the tumour; the hard bony case that covered it became gradually absorbed, and two small nipple-like projections, soft and pulsating, made their appearance. The pulse of the right radial artery, which had been becoming smaller gradually, was quite imperceptible at this period, and so continued to the last moment of life. A slight difference was observed in the size of the pupils at this period, the right being the larger; the difference became gradually less and less up to his death. The growth of the tumour was slow. The only complaint he made was of pain in the tumour, and shooting down the right arm; there was no dyspnea, no dysphagia, and, in short, there was a less amount of suffering than was usually met with in similar cases.

On the night of the 21st of March he had three severe fits of coughing, and great difficulty of breathing; so much so, that the patients in the ward thought he was dying; and the next morning the tumour, which had been in a very quiescent state for a considerable period, had increased from the day before to nearly double its original size; and it increased, not so much in prominence, as in the extent of the base. For the first time, on the 22nd, a distinct stridor—unaccompanied with dysphonia—was observed. Up to this period there was no suffering from dysphagia, but now he could only take food in a fluid form; there was no difference in the intensity of the respiratory murmur in either lung. After these severe fits of coughing he could no longer lie down; and during the last few days of his life he was obliged to sit upright. He had a short attack of delirium after one of these paroxysms, but was quite tranquil for the last two days of his life. On the 28th he was suddenly attacked with severe cough and dyspnea, became pale, then livid, and covered with cold sweat, and after two or three of these attacks he died.

On making a *post mortem* examination, the arachnoid was found slightly thickened and opaque, and a slight amount of subarachnoid effusion was found; but what was particularly interesting was, that the left hemisphere of the brain was smaller than the right, and a small depression or cicatrix existed on its surface on the inner and anterior part of the anterior lobe; small atheromatous deposits could be seen on both internal carotid arteries; the left carotid was impervious from its origin in the aneurism to its bifurcation.

On opening the chest, and removing the whole contents of the thorax, he found a spherical-shaped tumour, which sprang from the upper part of the transverse portion of the aorta; it measured sixteen inches in circumference. Neither the ascending nor the descending aorta seemed to be altered in their calibre; the tumour separated the vessels at each side, which arise from the aortic arch, to a great extent; the trachea was

completely flattened and arched, so as—over the most projecting part of the tumour—to have become convex posteriorly.

There was so much pressure exercised upon the trachea, that it was strange that the patient did not suffer more and earlier from difficulty of breathing than he did. The recurrent, pneumogastric and phrenic nerves could be traced, and no extraordinary pressure seemed to have been exercised on them. The impulse of the heart and its sounds were always feeble, but there was no abnormal sound; the heart was small, but healthy, with the exception of some little calcareous deposit about the semilunar valves, but it did not seem to have affected the function of the valves.

The attention of the society was directed to an interesting point connected with this case. The lungs were unusually dark, as also were the glands about the roots; at each apex there were small crude tubercles, and in both lungs several small cavities existed.

Dr. Banks considered the case was one of much interest, and it was a great satisfaction to him to be able to trace it, by Dr. Moore's very accurate report, from a comparatively early period. How long the disease existed it was not easy to ascertain; the earliest observation dates from 1861, when the case first came under Dr. Henry Kennedy's notice. The man was never very clear or very precise in his statements, so that no accurate knowledge of the early history of the case could be acquired; but it was interesting to observe the extraordinary remissions which occurred, according to the recorded history of the case. That this disease must have stood still repeatedly, and that the tumour must have at times altered its course, was clear from the fact of the left radial pulse having almost disappeared; and subsequently, there being no interference with the left, the right becoming almost obliterated. Another extraordinary fact was, the extreme rapidity with which the tumour increased in one night, in consequence of the frightful attacks of paroxysmal cough.—*April 1, 1865.*

*Aneurism of the Aorta.*—DR. MINCHIN communicated the following case:—

E. B., a strong, healthy-looking woman, aged forty-four years, was admitted into the hospital of the North Union, under my care, on the 3rd of January, 1865, with a large pulsating tumour occupying the anterior part of the neck. On the day of her admission she stated, that she first perceived it about two or three months previously. The account which she gave of its origin was very vague and unsatisfactory; indeed, all that could be ascertained amounted to this, that she had laboured under cough and dyspnea for some time; that the tumour appeared to be in some way connected with these symptoms, and that she had been in the Whitworth Hospital for a short time in the early part of the last



Winter. On making inquiry at that institution, I was enabled to ascertain the following particulars:—Admitted into the Whitworth Hospital on the 1st of August, 1864, complaining of slight orthopnea, which had continued for four months; at that time the anterior part of the neck was occupied by a tumour, which extended transversely to about an inch beyond the sterno-clavicular joint at each side, but did not form any great prominence anteriorly; it was distinctly pulsatile; no murmur was audible. She was discharged on the 11th of August.

When I first saw the case, in January, the tumour extended vertically from the lower border of the larynx to about an inch and a half below the upper border of the sternum; transversely from the middle of one clavicle to the corresponding part of the other; it formed a remarkable protuberance anteriorly, presenting in this direction a smooth rounded prominence, on which the chin of the patient rested in the semi-recumbent posture, which she usually maintained. To the hand applied over the surface of the tumour a strong impulse was communicated at each systole of the heart; on applying the stethoscope, a distinct bruit was audible. The pulse in the right radial artery was a little smaller than that of the left, and had a thrilling vibratory feel, which was not present in the vessel at the left wrist. Vesicular murmur was audible throughout the chest; no abnormal sound in the cardiac region; the heart's action was perfectly tranquil, eighty-six per minute; no venous turgescence in any part; no dropsy; very slight hoarseness, hardly amounting to dysphonia; no dysphagia whatever; occasional cough, accompanied with scanty mucous expectoration; some degree of orthopnea; respiration difficult in the supine posture, but tolerably free while she lay on either side, or when the body was raised to a half-sitting position; on sitting upright the breathing became perfectly easy.

For the next two months no new symptom was observed; the cough had become very much alleviated, and little or no distress was complained of. On the first of March part of the anterior wall of the tumour corresponding to the space between the sterno-thyroid muscles was observed to be growing thinner, and at this part a small secondary tumour projected, the size of a large almond; its surface was smooth and shining, but not discoloured. It continued gradually to enlarge until, on the 18th, it had attained the size of a plum, of a dark purplish-red colour, tense, extremely tender to the touch, and appearing as if it would burst upon a very slight exertion. On the evening of the 19th several minute openings formed in the skin which covered this globular projection, and blood and serum commenced to trickle from it rather freely.

March 27.—Some smart hemorrhages have taken place from time to time; the surface of the central tumour has a sloughy appearance, not very unlike an anthrax; several openings have formed, each of them large enough to admit a goose quill.



28th.—An opening as large as a sixpence has formed at the lower part of the circular sloughy surface; this opening is firmly plugged during each systole, but the plug is loosened during the heart's diastole, so as to admit a trickling of blood to take place. The patient has lost between twelve and sixteen ounces of blood since yesterday, judging from the appearance of the bedclothes. Pulse 98, soft; countenance pale, not anxious; decubitus nearly supine.

29th.—Lost nearly a pint of blood since last report. A circular piece of sloughy integument has separated, leaving an opening bounded by a well-defined ulcerated margin; into this opening a firm fibrinous coagulum is pressed forward so as to plug it up with remarkably accuracy; so closely is it fitted against the circumference of the opening, which is fully as large as a crown piece, that very little blood escapes at each diastole, and none whatever during the systole. I was sent for again at 6 p.m., and found that profuse hemorrhage had occurred. The attendant showed me half a basinful of blood, in addition to which the bedclothes and the floor were quite saturated. I found the patient lying on her back; countenance extremely pale; no anxiety; no incoherence; pulse 104, very small, but regular; intense thirst was complained of, but no other uneasiness.

On the next morning, at about 8 o'clock, the coagulum became suddenly displaced, and was shot through the opening to which it had applied itself; it was projected with considerable force, and followed by a large gush of blood, which was immediately fatal.

Dr. Minchin, on exhibiting the morbid specimen which had been removed from the body, remarked, that the almost complete immunity from suffering which the patient had enjoyed was to be explained by the fact that the aneurismal sac, during the progress of its growth, exerted no injurious pressure on any important organ. The point whence the aneurism had sprung was the transverse portion of the aortic arch, between the origins of the arteria innominata and the left carotid; from this point the tumour enlarged gradually upwards and forwards.

In making its way towards the anterior part of the neck (where the chief part of its bulk was situated while the case was under Dr. Minchin's care), the pressure of the dilated vessel had caused the absorption of a portion of the upper edge of the sternum, and had also induced a very considerable relaxation of the sterno-clavicular joint at either side. The tumour presented a very remarkable flatness at its posterior aspect. The œsophagus, trachea, eighth pair of nerves, and thoracic duct had not suffered any appreciable compression. The lungs were quite healthy; the heart was small, healthy, and firmly contracted; the fibrinous mass which had, by its pressure against the anterior wall, retarded in some degree the fatal issue of the case, was fully as large as a man's closed fist, and was exhibited to the society.—*April 1, 1865.*

*Disease of the Elbow Joint.*—MR. CROLY exhibited a drawing and specimen of ulceration of the cartilages of the elbow joint, removed by resection. The patient, a man aged fifty-four, was admitted into the City of Dublin Hospital under his care several months ago. A large ulcer existed in front of the joint; and there was a sinus, on introducing a probe into which, diseased bone could be felt. The destructive process, however, seemed to be limited to the joint, and there was no thickening of the periosteum over the end of the humerus. On pressing the radius and ulna against the humerus, the man complained of pain, but moderate extension caused no uneasiness. He excised the joint by making a single long incision, and turning out the ends of the bones. The cartilage was loosened from the capitulum of the humerus, and on raising it up the bone was seen superficially ulcerated; a thin slice of it was removed, and the osseous tissue thus exposed was found to be perfectly healthy and firm. The cartilages of the ulna and radius were similarly affected, being in some places detached, in others removed, and the bone being slightly ulcerated. Since the operation, the case had proceeded most favourably; the night sweats, and other hectic symptoms which had existed, having entirely ceased, and the patient had regained strength.—*April 1, 1865.*

*Fracture of the Ribs.*—PROFESSOR MACNAMARA said, the specimens which he now presented to the society were taken from the body of a man who fell about forty feet, from a house in course of erection, on Wednesday last, and was brought at once into the Meath Hospital. He presented the aspect of a person suffering from internal hemorrhage; and a very slight examination was sufficient to show that a number of the ribs were fractured. There was a very slight amount of emphysema, so slight that it entirely escaped observation, and even at the *post mortem* examination it was scarcely noticed. The man died soon after admission; and on a *post mortem* examination they found, on opening the thorax, that the right pleural cavity contained about forty ounces of blood. Eight ribs were fractured at the junction of their anterior and middle third—from the second down to the ninth, inclusive—and three others, close to their vertebral articulations. The intercostal arteries were lacerated, and the ribs projected into the pulmonary tissue, producing a large rent.

The most interesting circumstance, however, connected with the examination was, that the liver was lacerated, and yet there was no hemorrhage into the abdomen; on the anterior surface there was a slight fissure, but the under surface was rather extensively torn, and yet there was not a drop of blood found in the peritoneal cavity. He was aware, from a case that he had an opportunity of examining some years ago, that the liver could be extensively ruptured, and great quantities of blood poured out, and that after the reception of the injury the patient

could walk home and live for twenty-four hours, but he was perfectly unprepared to meet a case such as this was, of rupture of the liver without the effusion of blood.—*April 1, 1865.*

*Pulmonary Apoplexy.*—DR. DUNCAN said, that on that day week he brought under the notice of the society a specimen of pulmonary apoplexy, associated with disease of the heart, hemoptysis having been present during the progress of the case. The blood was scanty in amount, of a bright florid colour, and intimately mixed with air globules; it seemed to come from the capillary vessels of the lungs. The hemorrhage did not present what he believed to be the usual characters of the hemoptysis noticed in pulmonary apoplexy, but rather resembled that often associated with pure pneumonia, a condition which was present in the case.

In the instance to which he now directed the attention of the society, hemoptysis also was present, but the blood differed in colour and character. The case was one of phthisis occurring in a middle aged woman, and of three years' duration. She came into hospital on the 4th of March, suffering from hemoptysis, and an examination of the chest showed that vomicæ existed in both lungs. The hemorrhage was considerable in amount, the blood being of a dark colour, and seemingly mixed with purulent matter; it did not, however, gush out in large quantity, but seemed to be a general oozing from the mucous surface. She sank rapidly, exhausted by the bleeding.

Upon examination after death, large abscesses were found in both lungs, and one of them contained a large coagulum of blood; but, notwithstanding a very careful examination, its source could not be traced to any large vessel—confirming the opinion that had been formed during the progress of the case, that the bleeding was not due to the giving way of a vessel of magnitude, but that the cavity of an abscess gradually filled with blood, which became mixed with the purulent secretion.—*April 1, 1865.*

*Fatty Degeneration of the Heart.*—DR. JENNINGS detailed the particulars of a case of fatty heart, with general adhesion of the pericardium, and sudden death from pulmonary apoplexy. John Murray, aged forty, a man of unusually large and powerful frame, and who had some years since suffered from an attack of acute bronchitis, with congestion of the lungs, applied for admission into the Infirmary of the South Dublin Union on the 18th of March, complaining of cough, general malaise, and debility. So robust, however, was his appearance, so meagre his own description of his sufferings, and so ill marked (at first sight) the physical symptoms of any severe illness, that his application had almost been refused, when, on feeling his pulse I was startled to find it singularly weak, irregular, and fluttering. On examining his chest extensive dulness



was found to exist on the right side, particularly in the infra-mammary region; as also (but in a less marked degree), on the opposite side, where loud râles were generally audible. The heart sounds were singularly weak and muffled, but yet distinguishable, and unattended with bruit. The breathing was but slightly embarrassed, and he was able to maintain the recumbent posture without distress. His cough was accompanied with free expectoration of brown-coloured mucus.

In the course of the following week, under the use of stimulants, and extensive counter irritation, the dulness sensibly diminished, and the character of the pulse considerably improved, when suddenly, during the night of 1st April, he was attacked by severe hemoptysis, from the effects of which he rallied so imperfectly as to preclude all hope of ultimate recovery. On the 3rd of April a second and more profuse hemorrhage almost instantly terminated his existence. The blood was of a bright red colour, and formed a firm coagulum.

The autopsy disclosed the following condition of parts:—The vessels of the brain were completely empty, and its tissue very much softened, so much so that considerable difficulty was experienced in removing it entire: this softening was best marked on the cerebellar lobes. The anterior mediastinum contained much adipose deposit; and on withdrawing the margin of the left lung, which was emphysematous, and overlapped the pericardium, the latter was found covered by a thick coating of fat. On attempting to raise the pericardium, it was found universally adherent to the heart, and required considerable force to effect its detachment. The surface of the heart was also covered with fat, several lines in thickness, and which dipped down at intervals into its muscular structure. The organ itself was strikingly small, especially when the stature and muscular development of the patient were taken into consideration; it was also soft and flaccid, and the thickness of the left ventricular walls in particular were under the average; the valves were healthy. The right lung was intimately connected with the thorax by old adhesions, and the pleura was much thickened; morbid deposits existed in the aorta. Both lungs (but especially the right) were gorged with blood, affording a good example of diffused pulmonary apoplexy.

Dr. Jennings observed, that the immediate cause of death in this case was the infiltration of blood throughout the pulmonary tissue, induced chiefly, no doubt, by the atrophied and degenerated condition of the heart, but favoured also by the propulsive power of the already enfeebled organ being further impeded by the pericardial adhesion.

While all must admit the truth of what had been written regarding the occasional difficulty of clearly diagnosing the existence of fatty and weak hearts, yet it seems strange that that organ could have suffered from such extensive disease without a better marked expression of characteristic symptoms—such as great dyspnea, syncope, temporary paralysis, &c.,



from none of which had the man ever suffered. The only physical symptom during life was the feeble circulation; for, though his respiration had been somewhat embarrassed, yet it did not by any means amount to actual distress, and he had always been able to remain in the recumbent position. On the subject of the relation, as regarded cause and effect, between the adhesion of the pericardium and the atrophy of the heart, he was strongly inclined to consider the pericardial inflammation as the primary affection. The case was interesting for several reasons:—1st, as an unusually well marked specimen of double pulmonary apoplexy; 2nd, as illustrating the truth of Dr. Watson's teaching, that we are not always to estimate by the existence or extent of pulmonary apoplexy, abnormally violent action of the *right* ventricle, but rather the inability from disease on the part of the *left* to propel its contents; and, lastly, as corroborating (if corroboration were necessary) the statement of Dr. Stokes, that those physicians go at least too far, who assert "that general adhesion of the pericardium invariably induces hypertrophy of the heart, and generally hypertrophy with dilatation;" for here, where there existed a condition of the heart most favourable for the development of such a state of parts—where hypertrophy with dilatation might most naturally have been expected, the organ was unusually small; and in the present case it really seemed as if the support derived during life from the adhesion of the pericardium, was the very cause which prevented the expansion of its chambers.—*April 8, 1865.*

*Ulceration of the Intestines.*—DR. DUNCAN said the identity or non-identity of typhus and typhoid fever is a question in pathology not yet fully settled. I believe, when we meet with well-marked and characteristic cases of typhus and typhoid, the phenomena are so different, that there can be little difficulty in arriving at the conclusion that they are distinct diseases; but every case we meet with is not attended with the characteristic symptoms that properly belong to it, and then we are driven to the necessity of supposing that either the symptoms of two different diseases are united in one case, or else that the two fevers are really identical, and that the differences in the symptoms commonly observed depend on some accidental circumstance. Every case, therefore, which throws light on this subject ought to be brought before the profession.

About this time twelve months there was a man in the Adelaide Hospital labouring under well-marked typhoid fever; the symptoms, progress, and duration of the case established this beyond doubt. He went out convalescent, and within a week returned, presenting an entirely different set of symptoms, closely corresponding with what is observed in typhus as distinct from typhoid fever. If the two fevers are identical, and the latter attack only a relapse or renewal of the original disease, how was it that on his re-admission the patient presented symptoms so essentially

different from those exhibited at first when he was labouring under typhoid fever? If the two forms are different, it is easy to understand that when convalescence took place the man might, from exposure to the second form of fever, catch a new infection, and come back to the hospital labouring under it. This was what actually occurred. Lying in the next bed to him at the time of his original illness there was a patient in typhus, presenting a well-marked contrast in all its phenomena to those under which he laboured; and it is reasonable to suppose that, when the violence of his own fever subsided, he took, by proximity, the infection from his neighbour. The specimen I have now to show you is exactly the converse of this. It occurred in a woman, admitted to the Adelaide Hospital on the 1st of April. She had been an inmate of Cork-street Fever Hospital, where she had a very severe attack of typhus, lasting about three weeks, requiring the free use of wine and stimulants. From this she was transferred to the House of Recovery; and after remaining about a week at home she was admitted into the Adelaide. On her admission she had not the characteristic symptoms of typhus; she wanted the peculiar depressed look that so significantly marks that affection. The intellect was clear; there was no delirium; the tongue was clean, and there was an absence of that prostration and depression of the nervous system that is so characteristic of typhus fever. She had an eruption of a rose colour over the abdomen, but more abundant than what we usually meet with in typhoid fever; and, instead of being confined to the abdomen or chest, as it generally is in this disease, she had spots on the forearm. It was distinctly a rose-coloured rash, disappeared upon pressure, and came out in successive crops on the surface; all which point to typhoid fever as their source. At first some of these spots were ambiguous in their character; they seemed vesicular, and suggested to us the idea of its possibly being an anomalous case of small-pox not yet fully developed. This idea did not turn out to be well founded; there was no eruption on the face, and, with the exception of one or two slightly vesicular spots which did not enlarge, the rest of the eruption was distinctly measly in its character. The patient, notwithstanding these resemblances, wanted some of the symptoms of typhoid fever; there was no tenderness in the ileo-cecal region, and no diarrhea. On the 4th instant, seven days after her admission, we observed that her mind began to be astray; but the delirium was totally different from the delirium which we meet with in typhus. Instead of being the muttering, low, depressed delirium, it was more active and excited, and the woman was with difficulty kept in bed. Notwithstanding the free administration of stimulants, she sank upon the night of the 6th.

We were anxious to ascertain the state of facts about the ileo-cecal region, as upon this would depend the true estimate of the nature of the affection.

On examining the body, we found two patches in the alimentary canal

in which Peyer's glands had sloughed away, leaving the muscular coat underneath smooth and bare. I think, then, that in this case, notwithstanding the absence of many symptoms usually esteemed characteristic, such as diarrhea of a peculiar character (the feces in the intestines did not correspond with those of typhoid fever), and notwithstanding the absence of tenderness in the ileo-cecal region, we are justified in considering it as a case of typhoid fever—not a relapse of the original typhus, but a fresh infection of a different form.—*April 8, 1865.*

## TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

DR. BEATTY, President.

DR. HALAHAN *read the following paper on Pneumonia.*—In the first year of the existence of the Medical Society of the College of Physicians I may well preface the remarks I am about to lay before you by wishing the new-born child a long and prosperous life, hoping that its labours may be successful and instructive, and an ornament to that college of which it may fairly be said to be the offspring. May the phoenix which has arisen from the ashes of the dead society soar with stronger wings towards the sun of science, and add another to the many proofs of the skill and research of the Irish School of Medicine. To proceed, however, to the immediate subject of my paper. I propose to lay before you three cases, lately attended, of inflammation of the lungs, which I was enabled to bring to a successful termination. My object in calling your attention to them is not with the desire of establishing any new theory or of suggesting any novel practice, but solely to inform you how a certain treatment has succeeded in the particular cases to which I am about to refer. The treatment is certainly not new, as I became acquainted with it when the resident of the Meath Hospital, more particularly on my rounds with the late John James Parr, of whose skill and judgment the greatest light of Irish medical science entertained the highest opinion; a treatment afterwards confirmed in my mind by its adoption by the late lamented Dr. Robert Mayne, under whose instruction I had the honour and privilege of studying disease for six years. There are few subjects on which a greater difference of opinion exists than upon the treatment of disease; yet differ as we may, and think what we can, and say what we will about the different modes in which we may treat the same disease, on one point we are all agreed—that our greatest energies and best exertions are to be put forth to save life; and I am fully convinced that in the majority of cases the true disciple of medicine acts on no other principle. It would be well for the profession if we would grant to



another the charity which all individually ask for themselves—the admission that he has done what he *has* done from the noblest motives. That if one of our brethren fail where we think (perhaps conceitedly) that we would have succeeded, we should rather pity than condemn the unsuccessful practitioner, remembering that failure is not confined to the tyros of the profession, but that “that fell sergeant death” is often more than a match for the most experienced and trusted members of our great profession; did such a feeling exist among us, did we regard each other with a more friendly and trustful sympathy, did we help each other by discarding our miserable professional jealousies, and care more for the general credit and honour of the profession than for our own special advancement, we would elicit from the public not merely a respect based upon the consciousness of their dependence on our efforts, but one unalloyed by any feeling of distrust, inspired (not unnaturally) by the startling differences so often existing between men of confessedly equal professional standing. With these remarks I beg to lay before you the outlines of the cases to which I have already referred, not making mention of any particular medical, but merely giving you the stimulating treatment, as I believe in it depends our entire success; that it matters little whether we blister, give mercury, antimony, or any or all the medicines in the *Pharmacopœia*, if we support our patient sufficiently to bear him through; and it signifies not what disease we have to deal with if we fail to support our patient’s strength fully our efforts will be of little avail.

The first case was a gentleman labouring under the disease in the right lung; pulse 130; ordered beef-tea; port wine twelve ounces, one ounce to be given during night and day every second hour if awake; on the second and third days the wine was increased to eighteen ounces, and at night, about ten o’clock, a good strong tumbler of punch. On the fourth day the pulse was reduced to 80; on this and two following days received twelve ounces of wine; sixth, seventh, eighth, ninth, and tenth days, ten ounces. He was blistered, and upon the first visit was ordered a little grey powder, with James’, together with a mixture containing carbonate of ammonia, syrup of smilax, tincture of henbane, and camphor mixture.

The second case was a lady; right lung affected; pulse 120; ordered beef-tea; ten ounces of port wine to be given in the twenty-four hours; the same amount on second and third days; pulse 90 on third day; from the fourth to the sixteenth day eight ounces daily, together with good nourishing diet. She had mustard poultices frequently applied to her chest, and took the carbonate of ammonia mixture.

The third case was a lady, and it was late in the evening when I first saw her; the right lung was the one affected; pulse 140; ordered her to have at once a strong tumbler of punch, and to get ten ounces of wine every second hour during the night if not asleep; on second, third, and



fourth days twenty ounces of wine, together with strong beef-tea; on these evenings she received the punch and chicken broth. On the fourth day the pulse was 80; from this to the twelfth day the wine varied in quantity, but never less than twelve ounces daily. She had repeated mustard poultices applied, and took a little blue pill, James' powder, and extract of henbane every three hours for three days. It did not affect the gums. She, at the same time, took the carbonate of ammonia mixture, which she continued after leaving off the pills.

Having detailed to you these cases, I beg to make some remarks upon the treatment which resulted in a favourable issue. There are, as we all know, two great theories afloat as to the manner in which disease should be treated, put into plain English—one strives to *wrestle* with the disease, the other, with the greater skill, to parry its thrusts, and eventually wear it out. The former treatment is not inaptly (and I believe often not unfairly) represented in the allegory of a great French writer; he compares the physician who rushes head foremost at the malady he was called in to cure, to a blind man armed with a club; before him lay disease and nature; the blind man strikes out boldly before him; if he smites the disease he kills it, but if he unfortunately strikes nature he as indubitably slays her. The first system is the Bonaparte of medicine, who is overthrown in a Moscow collapse; the second the Wellington of the same, who, just as the sun is setting, wins his Waterloo. The first has, undoubtedly, the support of an old and able medical literature, and the recorded practice of ages; but the *success* attendant upon medical exertions during the last quarter of a century is as remarkable as the *failures* which signalized the age which produced the literature and recorded the practice.

The honour rests with our native land, and with this our native city, of having originated a revolution in medical treatment, of which the results, great as they are, are but the earnest of a marvellous future—a revolution which emancipated our profession from the trammels of an authority that had little recommendation but its age, which opened up for them another track than that trodden hardly down by a long line of predecessors. "He fed fevers;" the expression involved the intuition of genius, and the result of profound and experienced thought. What is its meaning? Is it that we should force animal food upon a patient whose stomach revolts against it? The question is unworthy of an answer. It means that we should feed fevers with the food which fever will accept—wine, brandy, stimulants of all kinds, to any extent which the patient will receive, or may be forced to accept. I have no doubt that in this lies the true secret of successful practice, not only in fever but in all diseases of an inflammatory type. Let us watch the disease, ready to meet the possible complications which may arise, keeping a special watch on the organs or functions most threatened. Let us act like a wary general,

who in making his approaches to a besieged town, he allows not his attention to be drawn off for one moment from the dangerous points from which at any moment the enemy may sally, but all the while he keeps his trenches full of armed men, ready at once for onslaught or defence. The trenches which we medical strategists attempt to occupy, in our attack upon the fortress of disease, are the vital organs from which spring forth the strength and life of men; and the stout soldiers which in our peril we summon to defend them are the whole army of stimulants, from the second letter of the alphabet to the last but three. Permit me to notice an objection against the stimulating treatment often urged by practitioners of the drench-and-drain school of medicine; they tell us that stimulants give but an artificial and transient strength. Now the expression *artificial strength* is a contradiction in terms; it is either *strength* or it is not; and a *transient* strength may be quite enough for *transient* purposes, which is after all the great purpose we have in view in tiding a patient over an attack of disease. There is in him a certain mysterious influence, deadly in its intentions, wasting in its assaults; it has, however, but a certain course to run, often the shorter in proportion to its virulency. Surely we are not astray in serving a *transient* purpose if we succeed in making the patient able to bear its blows, and to weather and wear it out; nor need we trouble ourselves about the *artificial* character of the strength our stimulants impart if it is enough to serve the purpose we have at heart—the saving of our patient's life. In connexion with the above, and in conclusion, I wish to draw your attention to one special point in the regard to which I believe the full success of the stimulating treatment may often depend—I mean the watching of the patient closely throughout the night, and the steady continuance of the treatment during those hours when we are most likely to neglect it or think it unnecessary, unless sleep, “nature's soft nurse,” renders interference of any sort unnecessary; we should be, however, specially cautious in watching the sleeper, availing ourselves of waking moments to pour in the stimulants; the neglect of one night, the stoppage for so many hours of invigorating treatment, and those hours confessedly the most depressing of the twenty-four, may undo all the efforts, however strenuously sustained, of watchful and anxious days. There can be little doubt that in the great majority of cases men die between midnight and dawn; connecting this fact with another—that the midnight hours are those, unnaturally enough, the patient is most neglected, we can hardly fail in perceiving a connexion of cause and effect between them.

It would ill become me to close a paper which refers, however crudely, to the stimulating treatment of disease, without expressing the reverence and respect which I feel towards the memory of that great man, who in that hour when fever stalked throughout the land alone perceived the true mode of dealing with the pest—“*Magna est veritas et prævalebit.*” In

that age (perhaps after all not very distant) when medicine shall have reached the position of a fixed and certain science, no name shall stand forth more illustriously amongst those who were foremost in effecting this great result than that of Dr. Robert James Graves.

DR. HAYDEN *on the Rhythm of the Heart's Action*.—At the outset I beg to observe, what, no doubt, has been likewise the experience of others engaged in a similar inquiry, that in prosecuting the study of cardiac pathology I have found my progress much impeded by the want of a clear and precise definition of the rhythm of the heart, and of an accurate appreciation of the relative position in time of the several phenomena which, collectively, constitute a cycle of the heart's action.

The determination of the gravity or diagnostic value of morbid sounds associated with the movements of the heart, depends, not so much upon their quality, or the site at which they are generated, as upon the cause of their production, and the interpretation of which they are susceptible in relation to the flux or reflux of the blood through the auriculo-ventricular and arterial orifices.

The first step in an inquiry of this nature must therefore be, to endeavour to determine the representative value of the normal sounds and movements of the heart, and to group them in the natural order, on the basis of the vital and physical changes which they involve, in such a manner that the mind may at once, and without effort, apprehend the members of each group, their causes, and relationship to one another.

With this view it is that I venture to submit to the society the result of my observations in a department in which so much has been already done, but in which, as it appears to me, much still remains to be learned.

That great discrepancy exists among writers as regards the order and relative duration of the intrinsic movements, and even the sensible phenomena of the heart's action, a few references will suffice to show.

Cruveilhier says the duration of the contraction of the ventricles is double that of their dilatation; and that the latter was "sudden and instantaneous," and attended with great force, in a case of ectopia cordis observed by him in an infant.

Williams divides the whole period of a single pulsation of the heart into five parts, of which two are occupied by the ventricular systole, one by the ventricular diastole (during which the second sound occurs) and the two remaining by the period of rest.

Bellingham says "the systole of the ventricles occupies about one-half the period of an entire beat of the heart—that is, it is as long as the diastole and the period of repose taken together, and is double the length of the ventricular diastole."

Thus these two writers regard the ventricular systole as occupying



two-fifths and one-half the period of a complete action of the heart respectively; whilst one of them estimates the pause as covering two-fifths, and the other one-fourth of that period; but both agree in considering the ventricular diastole, as being only one-half the length of the ventricular systole; and Bellingham says the second sound is synchronous with it, and that "from the termination of the diastole of the ventricles (*i.e.* of the second sound with which it coincides), until the commencement of the next systole, the ventricles are in a state of perfect repose, their cavities remaining full but not distended."

According to Messrs. Barth and Roger, if the whole period be divided into six equal parts, the systole would occupy about two; the short period of repose, that is the post systolic pause, one; the diastole, one; and the long period of repose about two.

Lænnec says that a little more than one-fourth of the whole period is occupied by the contraction of the auricles, to which he attributes the second sound; a little less than one-fourth by the pause, and one-half by the systole of the ventricles; he adds that "every twenty-four hours the ventricles have twelve, and the auricles eighteen hours of quiescence."

Markham assigns to the first sound about two-fifths of the period of a cardiac action; to the second sound about one-half, or rather more; and to the succeeding pause about two-fifths or somewhat less; during this pause the auricular systole and the completion of the ventricular diastole take place; and the second sound, according to him, succeeds the first, without any well-marked intervening pause.

According to Carpenter the whole interval between one beat of the heart and the next may be divided into four parts, of which two correspond to the first sound, one to the second sound, and one to the period of repose.

From the foregoing it is manifest that no settled opinion exists on the subject of the rhythm of the heart, and that consequently original observation is not only invited but absolutely required.

The views which I shall now venture to lay before the society have been arrived at, partly from experiments upon inferior animals, but chiefly from observations on the sick in hospital practice.

As regards the debated question of the cause of the heart's sounds, I will only remark here, that I believe the first sound to be due entirely to sudden tension of the chordæ tendineæ and auriculo-ventricular valves, supplemented by the impulse of the heart, and modified by the force of the impulse, and by the quality of the media through which it is transmitted to the ear, namely, the parietes of the chest, and of the heart itself; that the passage of blood has no share in the production of this sound, beyond the pressure which it exercises upon the ventricular surface of the mitral and tricuspid valves, and the consequent tension of



these and of the chordæ tendineæ; and finally, that morbid murmurs are in no degree kindred to the natural sounds of the heart.

The second sound I hold to be due entirely to sudden closure and tension of the sigmoid valves.

The whole period covered by a single action of the heart is divisible into two parts of unequal length, corresponding to the *ventricular systole*, and the *ventricular diastole*; of this period one-third is occupied by the ventricular systole; and two-thirds by the ventricular diastole, the pulse being at the rate of ninety in the minute. The first sound and the impulse initiate the ventricular systole, and are immediately succeeded by a short period of silence, or a pause, during which the ventricular systole is continued; this pause, which corresponds to the "short pause" of French writers, I propose to distinguish by the title of "systolic pause," as indicating the state of the ventricles during its occurrence; it is of very brief duration—at the normal rate of the heart's action not calculable, but distinctly appreciable by the ear, and is a pause only in the sense that that portion of the ventricular systole which coincides with it is unattended with sound.

The second sound immediately succeeds the systolic pause, and determines its duration; the period of time covered by it is quite incalculable, but belongs to that assigned to the ventricular diastole. It is succeeded by a long pause, which I propose to designate as the "diastolic pause," in contradistinction to the former. This pause corresponds to the interval between the second sound and the first, and at a pulse-rate of ninety occupies a period of one-half a second *minus* the length of the second sound. During this pause the ventricles are undergoing dilatation, and the auricles contraction. There is, therefore, no pause, in the sense of complete absence of movement of any of the chambers of the heart—the so-called pause is a period of *silence*, not one of *inaction*.

During the period corresponding to the second sound and the diastolic pause, that is, to the ventricular diastole, the auricles are in a state of contraction; this is feeble and of an undulatory character till towards its conclusion, when it becomes quick and energetic, completing the distention of the ventricles, and is immediately succeeded by their contraction. This terminal portion, or acme of the contraction of the auricles I propose to designate the "momentum of the auricular systole," because then the auricles, having discharged a portion of their contents, have acquired their maximum power of contraction, and propel a further volume of blood with increased force and rapidity into the ventricles.

The forced and sudden entrance of blood into the ventricles during the momentum of the auricular systole serves the useful purpose of communicating to the ventricles the stimulus of distention, in response to which they immediately react upon their contents, and thus complete the cycle of the heart's action.

Dr. Gairdner speaks of the "contraction, dilatation, and rest of each of the cavities," thus recognizing a period of "rest" as distinct from dilatation. I cannot admit "rest" in this sense, because I believe that at no period during the active performance of its function is the heart in a state of complete repose, that is, neither of contraction nor of dilatation; much less can I admit with Lænnec that the auricles enjoy eighteen, and the ventricles twelve hours of rest out of twenty-four. The so-called "rest" is an acoustic phenomenon of a negative kind—it is a period of silence during which the systole and the diastole of the ventricles, respectively, is performed without sound or other manifestation of movement perceptible to the auscultator. If the chest of a living animal be quickly laid open, and the pulsating heart brought into view, it will be observed that during the diastolic pause, the auricles, more especially the left, are undergoing a superficial undulatory contractile movement in the direction of the ventricles, which, at the same time, are being gradually distended with blood. Thus, during this pause the "rest" is represented by the contraction of the auricles, and the dilatation of the ventricles. During the first sound and impulse, and the succeeding systolic pause, the ventricles feel firm and rigid to the hand, and relax only on the occurrence of the second sound; the auricles, during the same period, are in a state of maximum distention, manifestly because of the stasis of blood within them consequent upon the occlusion of the auriculo-ventricular passages. The systolic pause, therefore, corresponds to the terminal portion of the ventricular systole, and of the auricular diastole.

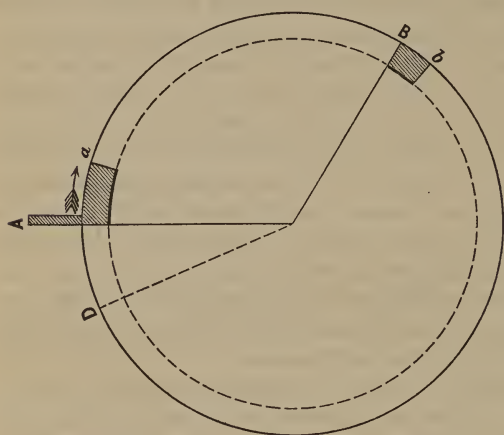
The radial pulse is usually spoken of as succeeding the first sound and impulse of the heart at a short but appreciable interval, as being interposed between the first and second sound, that is, as coinciding with the systolic pause. This is generally, but not universally true. I have found that in proportion as the rate of pulsation is accelerated the radial pulse is postponed, and when the rate amounts to 126 in the minute, the radial pulse exactly coincides with the second sound. I have not as yet had an opportunity of pursuing this enquiry further, but I conclude that, at a still higher rate of pulsation, as, for example, in febrile excitement of the heart, the radial pulse would be found to fall within the period of the diastolic pause.<sup>a</sup>

In regard to the effect of diminished rate of pulsation on the rhythm of the heart, it would appear that the diastolic pause is *relatively*, as well as absolutely prolonged thereby. I have had for some time past, and still have (May, 1865) under observation in hospital, a case of

<sup>a</sup> Since writing this passage I have had an opportunity of supplying the want here indicated. This morning, in the hospital dispensary, I had the opportunity of examining a patient in advanced phthisis, whose pulse was 144. On close examination I found, as I had anticipated from previous experiments, that in this case the radial pulse fell behind the second sound, that is to say, was postponed into the diastolic pause.

permanently slow pulse; it is that of a man aged seventy-five, with œdema of the feet and other symptoms of weak heart. The pulse is perfectly regular, and in the recumbent posture only *thirty* in the minute. The heart's action is equally regular. The sounds are somewhat dull but unattended with murmur. In this case, therefore, the heart pulsates once in every 2'', of which  $1\frac{1}{2}$ '', less the duration of the second sound, are occupied by the diastolic pause, and the remaining  $\frac{1}{2}$ '' by the first sound and the systolic pause. At a future meeting of the society I hope to have the pleasure of submitting the application of these views to disease, with illustrative cases.

In the accompanying diagram the circle represents the cycle of phenomena, commencing at



at the point A, and following the direction indicated by the arrow. The only phenomenon connected with the heart's action which makes itself sensible beyond the chest walls is the impulse; this is represented accordingly by the projecting line A. The impulse is accompanied and likewise succeeded by the first

sound, which in the diagram is represented by a coloured space (A to a) commencing with and extending beyond the line indicating the impulse.

The first sound is succeeded by the *systolic pause* (dotted line, a to B); and, as I have already observed, the duration of this pause is so short as to be separately incalculable by ordinary means when the pulse is at its normal rate of from seventy-five to ninety, but its existence is, nevertheless, readily perceived by the ear. A murmur *succeeding* the first sound, but not connected with the second sound, a phenomenon of not very uncommon occurrence in certain forms of mitral valve regurgitation, occupies the systolic pause.

The first sound, the impulse, and the systolic pause taken together, occupy one-third of the cycle, and are represented in the diagram by the segment from A to B, corresponding to the systole of the ventricles and the diastole of the auricles.

Immediately succeeding the systolic pause we hear the second sound, indicated, likewise, by a coloured space within the circle (B b); this sound is of the character of a sharp click, and is followed by a long period of

so-called rest, or rather of silence, which I propose to name the *diastolic pause*. This pause is represented by the dotted curved line (*b A*), and with the preceding second sound occupies two-thirds of the entire cycle, as represented by the segment B to A.

In point of fact, this long or diastolic pause is the only one which is generally recognized, the brief duration of the first or systolic pause having caused it to escape attention. During the continuance of the diastolic pause the ventricles are in a state of diastole and the auricles in a state of systole; it commences with the termination of the second sound, and ends only with the recurrence of the first sound, *i.e.*, the recommencement of the cardiac cycle.

Towards the conclusion of the diastolic pause, the auricles, which had been previously in a state of imperfect contraction, assume an increased vigour, and contract with rapidity and force; this portion of the contractile effort I propose to designate the *momentum of the auricular systole*; the auricles during that period performing a sudden, and, as it were, a spasmodic act of contraction. Immediately the ventricles have received the last charge of blood consequent upon, and resulting from this act of sudden contraction of the auricles, and have thereby acquired the stimulus of distention, which is essential to the contraction of hollow muscular organs, a reaction takes place, the ventricles contract in response to this stimulus, and we have a recurrence of the phenomena previously described. The portion of the diastolic pause corresponding to the momentum of the auricular systole is represented in the diagram by a segment marked off by means of a dotted line (D).

The entire cycle of cardiac action being thus divided into two unequal parts, one (A to B) corresponding to the ventricular systole, and the other (B to A) to the ventricular diastole, we find that the *first* period comprises three distinct phenomena, *viz.*, the first sound, the impulse, and the negative phenomenon, which I designate the systolic pause. The duration of the first sound I have not separately calculated. The impulse of the heart coincides with the first sound, but is not co-extensive with it; the first sound coincides with the impulse and extends beyond it. The systolic pause which succeeds the first sound corresponds with the last period of ventricular contraction.

If we follow the connexion between these phenomena still further, we shall find that when the pulse is at ninety, *i.e.*, when each cycle of cardiac action occupies two-thirds of 1'' the first sound, the impulse, and the systolic pause taken together, correspond exactly to a period of  $\frac{1}{4}$ '' . This it is not difficult to show with the aid of a time-piece beating *quarter seconds*. We shall find, moreover, that during the first sound and the impulse the auriculo-ventricular orifices are closed by the apposition of their valves, and the arterial orifices thrown open by the divarication of the two sets of sigmoid valves, and that during the systolic pause which



immediately follows the first sound, the same valves, both auriculo-ventricular and arterial, are in the preceding condition; the closing of the former valves and the opening of the latter correspond to the impulse and the first sound. The state of the circulation during the ventricular systole is, that the blood is passing from the ventricles into the arteries, the auricles being in a state of distention.

Connected with the ventricular diastole we shall find the following phenomena:—At the commencement of it the second sound, and immediately succeeding this the long or diastolic pause. The duration of the second sound is incalculably short; the diastolic pause is calculable, and when the pulse is at ninety it occupies  $\frac{1}{2}$ ", minus a minute fraction of time corresponding to the second sound. During the second sound the arterial valves are closed, whilst the auriculo-ventricular valves are suddenly thrown open, and during the succeeding pause the arterial valves continue closed and the auriculo-ventricular continue open.

The state of the circulation is, during the second sound, and the next succeeding and greater portion of the diastolic pause the blood is passing from the auricles into the ventricles, and the auricles are contracting feebly; they are performing that kind of undulating contraction of which I have already spoken as being witnessed in the heart of an animal recently killed, a peculiar wave-like movement which travels over the surface of the auricle in the direction of the ventricle. Towards the conclusion of the diastolic pause the auricles are in a state of energetic action, that peculiar and apparently spasmodic contraction, which I designate the *momentum of the auricular systole*.

As to the connexion between the radial pulse and the movements of the heart, it is commonly supposed that the radial pulse succeeds the first sound, and comes between that and the second sound. This, however, is true only when the pulse is at or near its normal rate. When the rate is under 126 the stroke of the radial artery corresponds to the systolic pause; but if the pulse amounts to 126 in the minute it will be found that the beat of the radial artery is shifted back in the order of phenomena, and is now contemporaneous with the second sound. If the heart's action be still further accelerated it will be found that the radial pulse is postponed in the same proportion, and now falls within the period of the diastolic pause.

The subjoined chart will be found to exhibit the connexion and mutual relationship of these several phenomena in more intelligible form.





PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.<sup>a</sup>  
 TWENTY-SEVENTH ANNUAL SESSION.

DR. CHURCHILL, President.

*New Instrument for Craniotomy.*—DR. HALAHAN, in exhibiting an instrument he had devised for this operation, made the following remarks :—

Sometimes in the practice of our great profession we are brought into contact with particular conditions of our patient, which require the employment at our hands of instrumentalities from which we naturally shrink, and which nothing short of the most absolute necessity would induce us to use. The absolute necessity lies just in this—that except we employ the means we shrink from our patient will *surely* die; *if* we employ them he or she *may* live. The real difficulty lies in determining *when* we are to act. Without precise examination we may presume the existence of a danger which is only imaginary, or fail observing obstructions which demand instant interference.

Craniotomy is pre-eminently an operation of this kind; and is one which, above all others, requires the consideration of many important points. First, is the fetus alive or dead. Secondly, if alive, is there possibility of preserving the life—possibility of preserving it without endangering the life of the mother. The preservation of that is of course the paramount obligation on us. She is to be saved at all hazards. But the true obstetrician will feel that he has done but half his work if he has slacked a single effort to save both. In operating it is quite plain that a great deal depends upon the instruments employed; our object should be to use as few as possible, and to render the operation as speedy and as simple as we can. In all cases of craniotomy we are at present obliged to resort to the use of at least two (which I believe fail in satisfying these intentions), the perforator and crotchet, and the employment of the latter is open to special and grave objections. We are to bear in mind the design we have in view in using it—to exert a full, steady, equitable traction power, without inflicting the slightest injury upon the vagina. I believe ~~that~~ the crotchet often fails in fully effecting either of these results. First, it does not effect a steady and equitable traction power, because it bears but on one point, the result of which shows itself in the fact—often but a bone is drawn away, when the head should be extracted. Secondly, it is *liable* to effect injury upon the vaginal parts; this is manifest from the fact that we are obliged to guard the point externally to the head when attempting traction. Under such circumstances

<sup>a</sup> These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.



an instrument that might be able to effect an equitable traction, without the obvious liability of the present crotchet to inflict injury on the parts concerned (as it is certainly a desideratum) would be well worthy of the attention of the profession. I beg to present to you, gentlemen, an instrument made under my direction by Mr. John White, of 68, Upper Sackville-street, which I anticipate will be found to combine the double offices of the perforator and crotchet—thus, at the very start, simplifying the duties of the operator, and enabling him to effect the desired result with a greater degree of certainty and success. The precise nature and mode of employment of the instrument I now beg to lay before you. First, it is calculated to effect perforation; and, having done so, by the action of a screw two arms are developed at right angles with the shaft, two inches from point to point. The advantage of the purchase, so to speak, of such a double traction, must be apparent. And in case any such unfortunate circumstance should arise, that the parts under traction should give way, you have the power of withdrawing the arms within the shaft, and removing the instrument.

As I believe the instrument which I now have the honour of exhibiting to be original in its design and operation, I may be pardoned the liberty of naming my own offspring, and beg to present to you, Mr. President, and to *you*, the honoured master of this great institution, Halahan's perforate-crotchet.—11th March, 1865.

DR. DE RICCI read a letter from Dr. T. H. Purdon of Belfast, detailing cases in which the employment of bisulphites had proved very beneficial.—8th April, 1865.

DR. FOOT read a report on the anatomy and pathology of the monster exhibited by Dr. Cronyn on 14th February.—8th April, 1865.

*Fatty and Hydatigenous Degeneration of the Placenta.*

Dr. JOHN A. BYRNE said:—

The cases which I bring before the society may perhaps be thought not undeserving their attention, inasmuch as one of them serves to illustrate the manner in which syphilis is sometimes communicated from the male to the female parent—viz., through the medium of the fetus—and they both serve to illustrate highly interesting changes to which the temporary nutritive organ of intra-uterine life is liable.

CASE I.—On November 7th, 1864, I was requested to visit a lady who had come from England. She had been married some time, and had become pregnant, but complained that her general health was not as good as it had been some time before.

On my visiting her I found that, according to her own calculations, she was about six months pregnant; but, indeed, there was some difficulty

in fixing the exact period, as she had forgotten the date of her last menstruation, and could not fix the period of quickening. However, from the size of the abdomen, the uterus extending to the umbilicus, and the fetal heart being distinctly audible, I thought she was about six months pregnant. She had enjoyed excellent health from the time of her marriage until about two months before my visit. She then began to experience pain of a lancinating character in the vagina, and about the anus, and she noticed some tumours which she thought were hemorrhoids. These caused her extreme pain and annoyance. She complained, also, of soreness of the throat and general *malâise*. There was a superficial abrasion of the right nostril, which excited my suspicion on account of its coppery appearance, and on looking into the throat I detected the characteristic ulcer on the tonsils. I suggested an examination, to which she most willingly assented, when I found the labia and the region about the anus studded with well-marked syphilitic tubercles. They were remarkably hard and dry, free from any discharge, and extremely sensitive. There were a few small copper-coloured blotches scattered over the arms and thighs.

There was no doubt in my mind now of the nature of the case; and I told the husband of the lady my opinion. He stated that he never had a primary sore, nor discharge of any kind, but that four years before his marriage he had a bubo which yielded to treatment, and that he never had any secondary or tertiary symptoms. He also told me that he had consulted a distinguished surgeon before he married, who advised him as a matter of precaution, to undergo a mercurial course, which he did; and at the time I saw him he had not the slightest trace of any syphilitic taint. Here, then, was a case in which the syphilitic taint was clearly communicated to the mother in an indirect manner, viz., through the fetus, as I have not the slightest doubt that the gentleman's account was correct. The lady was ordered a slight course of mercury, to which she was very impressionable, her mouth becoming sore on the second day from the administration of two grains of hyd. c. creta three times a day. Black wash was ordered locally to the tubercles, and she soon began to improve; and she was then ordered small quantities of iodide of potassium in sarsaparilla, and subsequently tonics and chlorate of potash, and about the 6th December she was apparently free from any traces of the disease, and her general health remained excellent up to the period of her confinement.

Her confinement took place at Kingstown, where I had ordered her for her health, on January 27th, when she gave birth to a male child, dead born, and apparently dead for about three weeks or so, just corresponding to the period at which she ceased to feel fetal movements, and which presented all the appearances which are characteristic of the fetus, which has perished and remained about this time in utero. The presentation was

breech, and the labour was very quick, and with the exception of slight hemorrhage before the completion of the third stage, there was nothing remarkable. The lady's convalescence was excellent, and there was well-marked mammary secretion on the third day, a circumstance which is often observed in cases where the children are born dead, and have been some time dead in utero.

On examining the placenta it was smaller than is usual. What struck me particularly about it, however, was the appearance of the maternal surface. Instead of presenting the usual fleshy appearance, this surface was smooth, glistening, and as if it had been converted into fat; and on making a section of it this condition appeared to pervade nearly its entire extent.

Such a complete degeneration of the placenta into fat as in this instance I have never seen before, although I have seen many cases, as most of the gentlemen here, who have much experience in such matters, I am sure have, in which partial deposits of fat, or fatty-looking substances, took the place of the natural tissue of the placenta; nor do I remember ever having seen a case in which this development of fat existed to such an extent as to warrant me in attributing to it the death of the fetus, or to look upon it as the effect of the death, and I regret very much that circumstances have prevented me from exhibiting it here this evening, but its original appearance was very soon lost. I, however, had a section made by my friend Dr. Cryan, who kindly exhibited to me the microscopic appearances, which very much resembled those delineated by Dr. Hassall in the paper which has been published by Dr. Barnes.

The second case, in which I was able to trace the connexion of syphilis with placentary degeneration, but of a different kind, viz., hydatidiform, or vesicular degeneration of the chorionic villi, I shall briefly describe. A young woman, whom I had treated for primary and secondary syphilis, and who had contracted it from her husband after the birth of her first child, again, after weaning her child, became pregnant. When, according to her own calculations, she was about four months pregnant, she again consulted me, and complained that she suffered from great sickness of stomach, swelling of the feet and legs, and great increase of size of abdomen. On examining her, I found that the abdomen was much larger than what should be expected at this period—the uterine tumour extended, in fact, to the umbilicus, and the lower extremities were anasarcaous. I could not hear any bruit nor fetal heart sounds. In a few days afterwards she was attacked by uterine hemorrhage; and this was followed in seven or eight days by the expulsion of a large mass of vesicular growths. There was no trace of a fetus, but the expelled ovum presented all the characteristic features of this degeneration so well described by Mettenheimer, Hewitt, Barnes, and others.

She recovered perfectly, and has since become pregnant.

Dr. Byrne here exhibited the hydatigenous preparation.

Although Drs. Barnes, Druitt, Hewitt, and others have described this form of degeneration very accurately, as well as the other form, and have endeavoured to explain the causes which lead to such a wonderful metamorphosis as that which occurs particularly in the hydatigenous variety—and although they have detailed many examples of the occurrence of both kinds of degeneration, yet in none of their cases have they shown that it depended upon a syphilitic taint, although syphilis is laid down in all treatises upon obstetrics to be one of the most frequent causes of abortion. Perhaps in many of the cases detailed this change or degeneration may have depended upon this poison being in the maternal system; but the fact is not mentioned, and any new facts which would tend to illustrate the particular mode by which the disease may destroy the fetus in utero, may have the effect of eliciting the opinions of the members of this society upon this highly interesting subject.

In my own limited experience I have seen many cases of abortions, and also of children born dead and putrid, and some, also, of hydatidiform placental degeneration; and I have examined the placenta in numerous instances. But I have never seen the placenta so much altered in cases of dead and putrid children, or in abortion even at a late period, as I have seen it in the case I have just described. The usual appearance of it, in cases where the fetus had perished some time before delivery, was this:—It was smaller than it ought to be, softer, too, lighter in colour, and flabby in consistence; in some cases tubercular-looking particles, like small lumps of fat, were seen, and even in some placentas, which could not be but regarded as normal, these were occasionally to be found scattered through its substance. In some cases, too, in which the fetus perished previous to birth, no doubt, I have seen the maternal surface presenting an appearance as if it had been coated over with a tallowy substance, but in most of these instances it was very superficial.

Dr. Hassall says:—In the placenta affected with fatty degeneration certain of the lobes, in place of presenting the red spongy texture of the healthy tissue, exhibit a fatty appearance, and are of a yellow colour, glistening, firm, and ex-sanguinal, while the remaining lobes present their ordinary characters, at least to the unaided eye.

#### *Different Degrees.*

1. We observe that the villi are thickly studded with innumerable sphericle of oil.

2. The chorion is much altered; it is thickened, and destitute of nuclei.

3. The walls of the vessels no longer contain nuclei, these having, in all probability, degenerated into oil sphericles.

4. The sphericles of oil are contained some in the chorion, some in the



walls of the blood-vessels, and many in the intervals or spaces between these.

5. The cavities of the vessels are almost invariably free from fatty deposition.

6. The vessels are destitute of blood.

For our present knowledge of fatty degeneration of the placenta we are indebted chiefly to Dr. Barnes who, in Vols. XXXIV., XXXVI., of the *British and Foreign Medical and Chirurgical Review*, has largely discussed this affection or pathological change in an organ which is so essential in its integrity to the life of the fetus in utero, although he acknowledges that before his description Kilian described a case in which he dissected an ovum affected with fatty degeneration.

The present theories with regard to this transformation are two, viz. :—First, that propounded by Kilian, Barnes, and Hassall, which is, that the metamorphosis or change in the placenta, in both cases, viz., fatty degeneration and hydatidiform degeneration precedes the death of the fetus, and is in fact the cause of the death, either by producing hemorrhage, and thus cutting off the supply to the fetus, and necessarily exciting the uterus to expel the dead ovum, which acts as a foreign body, or if there be no hemorrhage, by the separation or rupture of the utero-placental vessels; by those fatty masses pressing upon the fetal villi, and by this obstruction eventually cutting off the supply, just as we see the vessels in other organs obstructed, and the supply to those organs cut off; and in this manner the fetus perishes through inanition, as it were; and thirdly, in some cases a combination of both causes leading to the expulsion.

Dr. Druitt, in a very able paper, whilst agreeing with Dr. Barnes as to the appearances of fatty placenta, takes a different view, and looks upon this change or degeneration as only one occurring in an organ manifestly lardaceous, and comes to the conclusion that traces of fatty or earthy degeneration are to be found in all *mature* human placentas, and in noticeable degrees in at least one-fourth. He says :—

1. And often incipient degeneration is a normal condition of the placenta at the end of pregnancy.

2. That it arises from partial cessation of the active functions of the organ, when the fetal development is nearly completed.

3. That when occurring in the *earlier months*, it probably arises from some antecedent want of nutritive force in the fetus, or by its death.

Dr. Grailly Hewitt, in a paper read before the Obstetrical Society of London, and published in the reports of that society, whilst agreeing with Cruveilhier, Mettenheimer, Gierse, Wedl, and others, as to the characters and appearance of the vesicular mole, dissents from Mettenheimer and Naegelè as to its origin. They look upon it as essentially consisting of cyst formation, and as resulting from transformation of the villi of the

chorion; but he is rather disposed to agree in opinion with Giërse, who thinks that the change consists in hypertrophy of the natural structures found in the chorionic villi, with secondary œdema. He then goes on to combat the opinion of Barnes, that the disease of the chorion is the cause, the death of the ovum the effect, in fatty and vesicular degeneration of the chorion, whose buds—to quote the words of Dr. Barnes—under the influence of a perverted developmental force, instead of growing into villi, may dilate into true vesicles or hydatidiform cysts; and Hewitt expresses an opinion diametrically opposite to that of Barnes, viz., that the changes or degeneration are *secondary* to and a *consequence* of the death of the ovum, and he advances some strong arguments in favour of this view. He says, that during the second months of fetal life the chorionic villi are in a state of great activity, supplying the means of growth to the embryo; and according as the fetus is growing some of these villi begin to be developed into the placenta, while the remainder undergo a gradual disappearance, the vitality of the fetus determining the further development of the placenta villi, and the development of the fetus and the placental chorionic villi proceeding *pari passu*. He says that there are no facts showing, that the embryo being dead the chorion villi can subsequently become developed into blood-vessels, as is the case when the life of the embryo persists; but when the embryo dies, this circumstance does not necessarily determine the cessation of vitality in the chorion villi; in order that this may be the case it is necessary that a separation be effected between the uterus and decidua. As long as the decidua remains connected with the uterus, the chorionic villi will continue to enjoy a certain degree of vitality; but the embryo, having ceased to live, this vitality is bereft of developmental power, and the chorionic villi, though they may actually grow and increase in size, retain the essential structure which they possessed when the death of the embryo took place. Accordingly, if the death of the embryo occur very early, and before the appropriation of certain of the villi to form the placenta has commenced, the ovum may continue to grow, and may remain in the cavity of the uterus, and in such a case the whole of the chorionic membrane would give rise to hydatidiform villi. If, on the other hand, the death of the embryo be postponed until the formation of the fetal placenta has commenced, the hydatidiform degeneration will be necessarily limited to that part of the chorion which is in contact with the decidua scrotina. He also says, that after the villi form the placenta they are no longer capable of undergoing the hydatidiform degeneration. Regular blood-vessels having taken the place of these processes, the conditions necessary for the production of these peculiar vesicular enlargements no longer exist, and he limits the period of this degeneration to the end of the third month at furthest—no hydatidiform degeneration occurring subsequently to this period, according to his opinion.

Now, I have in my possession a preparation of a withered blighted fetus, which had arrived to the fifth month at least of intra-uterine life, and which was expelled with a large quantity of vesicles. The fetus is well formed, but presents all the signs of having been dead in the uterus for a considerable time before its expulsion, viz., it is flattened, shrunken, and has the appearance of having been pressed for some time between two hard substances. In this case numbers of vesicles were discharged, and numbers are attached to the placenta and chorion; but the specimen shows that the placenta is formed, and the vesicular degeneration went on still. Dr. Hewitt accounts for a case somewhat similar to this, by stating, that a few of the villous processes, which at an early period of development completely cover every part of the chorion, had remained in this situation, and had been developed into the bodies.

So much has been written upon this interesting pathological condition by the advocates of both views of the question, and the subject has been so exhausted in the papers above quoted, that any arguments that I could bring forward could have very little weight; but the bringing forward of those two cases may perhaps elicit a discussion as to the views which the two different theories may have influenced the members to take—viz., whether placentary degeneration of either the fatty or vesicular kind is the cause of the death of the fetus, or whether it is the effect. My own tendency, I must confess, after attentively considering both views of the question put forward and supported by so many elaborate arguments, is to regard the explanation of Dr. Hewitt as the most satisfactory, although I have great respect for the opinion of so distinguished a writer as Barnes.—*8th April, 1865.*

DR. DENHAM exhibited a larynx taken from the body of a woman who died soon after confinement.—*8th April, 1865.*

*On Dysmenorrhea.* BY GEORGE H. KIDD, M.D., F.R.C.S.I., Assist. Physician to the Coombe Lying-in Hospital.

Painful menstruation is a symptom common to so many affections that to discuss the subject fully would be to enter on the consideration of the whole range of uterine and ovarian pathology. In fact, dysmenorrhea is but a symptom, and is no more entitled to a place in our nosological lists than is dropsy; but as dropsy is often the most prominent symptom, and one for which it is sometimes difficult to ascertain the cause, so painful menstruation is very often the symptom that makes patients first seek our aid, and on examination it is either found to depend on some organic disease, or it may be impossible to detect any definite cause for its production. Cases of this last description are, however, becoming more rare, and there are good grounds for expecting that as our knowledge

of uterine disease becomes more extended, we shall be able to trace all cases of painful menstruation to their cause, and by directing our treatment to the removal of this, make it more successful than it can claim to be at present. I do not mean to attempt then a full exposition of the causes of dysmenorrhea, but rather to relate briefly some of the more typical of the cases I have records of in my note book, with such observations as they may suggest.

The first case I shall mention is that of a young English lady, who came under my care in 1860, while on a visit with some of her relatives in Dublin. When I first visited her, I found her in great pain, her face livid and dusky, her features contracted, the surface of the body cold, the pulse small and quick; she lay writhing in bed, straining herself with ineffectual efforts to vomit, and every few moments dashing off the clothes, and throwing herself from side to side in paroxysms of pain. In this state she continued for several hours, notwithstanding the use of opiate enemata, hot fomentations, the exhibition of opiates and stimulants by the mouth, and ultimately the inhalation of chloroform; but this latter, I must say, I did not push to its full physiological action, being deterred from doing so by the state of the circulation, and the colour of the face. I learned from her at my visit the following day, that she had been in the habit of suffering in this way at each menstrual period for some years past, though not generally to the same degree as on this occasion. When she first began to menstruate she had no inordinate pain; but about three or four years after it commenced, and four or five before I saw her, she got a chill one day while menstruation was going on, by which it was suddenly suppressed. At the next period she had some pain, and the discharge was so excessive that medical advice had to be obtained, and for ten months after this she did not menstruate at all. Then the function was re-established, but was ever afterwards attended with great pain. During the interval she believed herself to be in good health. By the use of warm hip-baths, leeching at the anus, counter-irritation over the iliac regions, tonics and aperients during the intervals, and the early use of opiates when the pain set in, the three or four succeeding catamenial periods passed over without very much suffering, and this lady returned to England; but her sufferings soon became as severe as ever.

I next saw this lady in the month of November, 1862. She had then been married for some months, and had come to reside in Dublin. The pain was now even worse than it had been before; her general health was suffering, and she had, during the intervals, a good deal of mucopurulent discharge, which was often of a brownish-red colour, as if from being mixed with blood, and she often suffered a good deal of pain at the middle of the intervals between the periods, but without menstrual discharge. I now made a digital examination, when I found the uterus low



in the pelvis, soft, large, and retroflexed, the os so large as to admit the entrance of my forefinger. On introducing a speculum, I found the lips greatly congested, swollen so as to evert the lining membrane of the cervix, and appearing to be so softened that the fingers might be pushed into their substance with ease. From the interior of the cervix, a long string of clear ropy mucus hung down into the vagina; and, on attempting to remove this, the surface was very easily made bleed. The uterine sound passed freely, and the retroverted fundus was easily replaced, but fell back again on the sound being withdrawn.

Here, then, was a case of inflammation of the cervix extending into the canal of the neck, and the most prominent symptom, in fact, almost the only one complained of, was pain during menstruation; and certainly the constitutional disturbance produced by that pain was greater than it has ever been my lot to see under any circumstances, the system being brought, on many occasions, into a state resembling the collapse of cholera.

By the use of frequent leeching to the cervix, baths, counter-irritation, painting the cervix with strong tincture of iodine, and long-continued rest, with appropriate constitutional treatment, the condition of the cervix was improved, and the pain relieved; still the marriage remained unfruitful, and after some time all the symptoms returned, and on the 15th of September, 1864, I was again called to see her, when I found her suffering nearly as much as on any previous occasion. As soon as the catamenial flow was over, I examined the uterus, when I found the cervix again inflamed, and assuming the same spongy condition as before, which perhaps had never been thoroughly removed, and I determined, after subduing the acute symptoms, to try to modify the condition of the tissues by the deep action of the potassa cum calce. With this, after the lapse of a few days, I formed a deep eschar on each lip; and at the same time I passed the caustic into the interior of the neck, so as to act superficially on the mucous membrane lining it. When the eschars separated, the ulcers produced by the caustic healed rapidly, but required the occasional application of nitrate of silver to check exuberant granulations; the cervix became firm and small, the secretion of tenacious mucus from the cavity of the neck ceased, and the os closed to its natural size. The succeeding period passed without any pain whatever, and the lady became pregnant, and is now in her seventh month, and in the enjoyment of good health.<sup>a</sup>

The next case which I shall take from my note-book is an illustration of that form of dysmenorrhea in which the body of the uterus is the part engaged, the cervix often remaining healthy, or being but very slightly affected. Fortunately this is not so common a form of disease as that which existed in the preceding case, for, from the affected part not being

<sup>a</sup> Since this paper was read this lady has been confined, and is now in good health.

so accessible to treatment, we have much more difficulty in accomplishing its cure. I have met the disease under two forms—first, as an effect of deficient involution of the uterus after delivery; and secondly, as the result of chronic internal metritis. The following is an illustration of the first of these forms:—

Mrs. W., a widow lady, consulted Dr. Banks for palpitation, and a variety of dyspeptic symptoms; and he, tracing these to their true source, a uterine affection, placed her under my care. This lady had four children; her husband died from fever shortly before she was confined of her last child, who is now ten years old. She had a bad recovery from this labour, and was not able to nurse the child. Ever since this she has suffered from pain in menstruation, the usual course being to have pains for some hours before the menstrual fluid appeared; then the discharge came on scantily, and gradually increased in quantity, when the pain subsided; she now remained free from pain, but generally on the third day the discharge increased greatly in quantity; and on many occasions the hemorrhage was so great as to seriously alarm and weaken her. During the intervals of menstruation she suffered chiefly from palpitations and flatulent dyspepsia; but sometimes she suffered severe pain in the back, extending down one or other leg, making her almost unable to move—attributed, she said, by her medical attendant in the country, to a falling backwards of the uterus, so as to press on some of the sacral nerves, and relieved almost instantaneously, by him, by placing her in the erect posture, and passing his finger into the vagina and tilting the uterus forwards. On examination I found the cervix of the uterus a little larger than usual, but presenting its natural colour and consistency, with a smooth surface, and the os of a normal size. The sound passed into the uterus to the extent of four inches, being an inch and a-half more than it ought to do; and on placing my hand on the hypogastric region I could detect the body of the uterus very much enlarged, and tender to the touch; but the enlargement was uniform; there was no tumour in any part of the uterine walls, or localized swelling, which, with the history of the case, the absence of unhealthy uterine discharge, and the comparative freedom from constitutional suffering, induced me to attribute the enlargement to deficient involution after labour rather than to chronic metritis.

The hemorrhage, on the second or third day of menstruation, is, I believe, an almost constant symptom in cases of enlargement of the body of the uterus.

It will be observed that in both of the foregoing cases there was great pain before and at the commencement of menstruation, and relieved when the discharge flowed freely, thus resembling cases where there was a mechanical impediment to the escape of the discharge; and yet during the intervals the sound passed into the uterus freely, showing that the

canal was quite pervious. Two explanations of this have been offered. Dr. Grailly Hewitt and Dr. Meadows believe that though the canal is quite open during the interval of menstruation, it is closed at the commencement of the process by the congestion and erectile enlargement of the uterus that takes place at this time, and that the pain arises from the efforts of the uterus to expel its contents. Scanzoni, adopting a somewhat similar view, suggests that the abnormal nervous irritation of the uterus may give rise to reflex movements of the organ, and to a convulsive contraction of the cavity of the neck, hence the escape of the blood which is already effused, is impeded by an obstacle which irritates in its turn the motor nerves of the body and fundus of the uterus. "This, he says, is what to us appears particularly to take place in cases where after intense expulsive pains, lasting perhaps several hours, the patients suddenly eject a considerable quantity of blood, partly liquid, and partly coagulated, and find instantaneous relief."

Dr. West, on the other hand, attributes the pain altogether to the congestion, "the congested womb ached," he says, "till nature bled it, just as the head aches till the cupping glasses or the leeches have relieved the overloaded cerebral vessels."

My own observations incline me to believe with Dr. Grailly Hewitt and Dr. Meadows, that in these cases "there is a partial but temporary menstrual retention," and to attribute this, in many cases, to the mechanical closure of the passage by the erected and congested state of its walls; but very frequently a clot forms in the body of the uterus, and passing into the cervix obstructs the escape of the discharge, till it is forced by the uterine contractions out of the way. Acting on this view, I recommended the application of two or three leeches to the anus at the onset of the pain, in the case now under consideration, that by emptying the hemorrhoidal veins which have a direct communication with those of the uterus the pressure on these might be relieved; and the effect was most beneficial. The pain at the commencement of the process has been greatly and almost immediately relieved, and there have been no hemorrhages, so that instead of being weakened by the leeching, there has been a great saving in the amount of blood lost, and in the strength of the patient.

The administration of Indian hemp contributes to the same end, as it seems to lessen the tendency to hemorrhage, and acts at the same time as a sedative. A combination of ipecacuanha with sedatives is also useful; the nausea caused by the ipecacuanha, perhaps, serving to relax the spasm, or reducing the erection of the os. In some of these cases the warm hip bath, especially after leeching, and allowing the leech bites to bleed in the bath, gives more prompt relief than any other means. The enlargement of the body of the uterus, or the chronic metritis, must be attended to during the intermenstrual periods; unfortunately both

affections are very little amenable to treatment. In some cases of metritis the retention of the secretions within the uterus keeps up the irritation, so as to render all treatment useless till the passage is fully dilated. In one instance of chronic internal metritis, that I saw about six years ago, and that had previously been under treatment for years without benefit, I obtained the assistance of Dr. Beatty, and divided the cervix, with Dr. Simpson's metrotome, and from that time an improvement began in her symptoms; and though from her leaving Dublin I have lost sight of her, I have reason to believe the improvement has been permanent.

Did time permit, I would next detail a case in which chronic subacute cellulitis, or metro-peritonitis, the remnant of an acute attack supervening after labour was the cause of painful menstruation in a previously healthy woman; but I must pass over this, as well as the cases of painful menstruation in affections of the ovary, in chlorosis, and in the deferred menstruation of young girls, of all of which my notes afford good illustrations, to speak of that form depending on a mechanical impediment to the escape of the discharge from narrowness of the passage.

I believe this obstructed dysmenorrhea to be much more common than some of our standard works would lead us to anticipate, and that it is one of the most frequent causes of sterility. I have met with it as a congenital and as an acquired affection. In the congenital form the organs may all be quite healthy, with the exception of the narrowing of the passage, or there may, of course, be superadded disease. In the acquired form, I have seen it as the result of the contraction of cicatrices from ulcers, and from the use of the stronger caustics, also from the hypertrophy of chronic inflammation, and from the contortions of the canal caused by displacements of the uterus. In all of these cases the dilatation of the canal is an essential part of the treatment, and in uncomplicated cases the relief from pain is immediate. The dilatation may be produced by dilating tents, such as sponge, gentian root, or sea tangle—by the use of bougies gradually increased in size, or metallic dilators, or by instruments for dividing the sides of the canal, such as I exhibit.<sup>a</sup>

I have dilated the cervical canal of the uterus, in many cases, by most of these methods, all of which have peculiar advantages in special cases. In a case of acquired narrowing, the result of chronic inflammation of the cervix, that I attended with the late Dr. Montgomery, we dilated the passage by introducing bougies every day, for some weeks, gradually increasing their size; the narrowing was permanently relieved, and the lady who had been sterile for some years, soon afterwards became pregnant.

The sponge or tent of sea tangle affords a more rapid dilatation; but the

<sup>a</sup> Dr. Kidd exhibited the instruments used for dividing the os by Dr. Simpson, Dr. Coghlan, Dr. Greenhalgh, and Dr. Savage.



passage closes again nearly as rapidly as it had been opened. In one case of congenital narrowing, in which I assisted Dr. Sawyer to slit the os with Savage's hysterotome, he told me that it had been several times fully dilated with sea tangle, but without any benefit, and this fully accords with my own experience. When it is required to dilate the passage for temporary purposes, the sea tangle is a most valuable agent; but for the cure of dysmenorrhea or sterility, I believe it to be useless, unless it can be followed up by the daily introduction of bougies, or by keeping some such instrument in the passage for a considerable time.

Dr. Gream, of London, has recently written to the *Lancet* renewing a recommendation he had made in 1849, to use a metallic instrument with expanding blades, for the purpose of dilating the passage; others have also recommended metallic dilators, and the late Dr. Johns showed me wooden screws of a conical form, the invention, he said, of some French surgeon, which he was in the habit of screwing into the os for the purpose of dilating it; but I think the objection applies to all of these instruments that they do much violence to the uterine tissues, which may give rise to serious inflammation, and that the dilatation they effect is not permanent.

Dr. Simpson was, I believe, the first to adopt the plan of dividing the cervix, for doing which he used a *bistouri caché*. Having regulated by a screw the extent to which the blade may protrude, he guides the instrument, in the closed state, into the os with his finger, then presses on the handle, protruding the blade, and withdraws the instrument, dividing one side of the cervix in doing so. He then turns the instrument, re-introduces it, and divides the opposite side. He directs us, in performing this operation, to divide the os internum to a slight degree, and to slit completely the portion of the cervix within the vagina, so as to make a funnel-shaped opening. Nitrate of silver is then to be applied to the angles of the wound, so as to prevent union by the first intention, and the parts are to be painted over with a solution of perchloride of iron to prevent hemorrhage.

Our late President, Dr. Beatty, suggested the use of the four-bladed speculum in performing this operation; and in one case I adopted his plan, but I found it very difficult to make sure of the distance to which I had introduced the metrotome into the uterus, from the way in which its polished surface reflected the light, so that in subsequent operations I have always used Sims' "duck-bill" speculum, which enables one to see and touch the os at the same time.

Various objections have been made to this operation, both as to its details and to its principle. I shall first consider the improvements that have been suggested as to the details. In the first place, it is objected by Dr. Coghlan, of Wexford, who has written some valuable papers on the subject in the *Medical Times and Gazette*, that the incision is

unnecessarily large, and he has suggested the use of a cutting gorget with a probe point, to make an opening of a definite size by being pushed into the uterus, the probe point keeping it within the canal; he then introduces a roll of lead to keep the wound from closing; and he has published many cases successfully treated on this plan.

In using the instrument, the finger is placed against the os uteri, and the gorget glided up on the finger till the point enters the os. It is then pushed on in the axis of the uterus, dividing the cervix in its passage, to the extent of its own breadth only. Dr. Coghlan believes the probe point is sufficient to keep the gorget within the canal of the cervix, and that from the limited extent of the incision there is little or no danger of hemorrhage, while by keeping the roll of lead in the passage till the wound heals, he secures its remaining patent. I have never used this instrument, believing that there would be much risk of driving it through the walls of the uterus, and wounding the peritoneum or other important parts.

Dr. Coghlan devised the "dilator" I exhibit for introducing the lead. He rolls a thin piece of lead round the point of the instrument when in its closed state, and then introduces it well into the uterus, and expands it by opening the dilator, which he then withdraws, leaving the leaden tube in the canal of the cervix. In cases of slight contraction he uses the lead without any preliminary cutting, a practice that I believe to be often very useful, especially after the use of sea tangle or other dilating tents.

The next instrument I have to exhibit is that devised by Dr. Greenhalgh, of London. It consists of two blades concealed in a metallic sheath. It is introduced into the uterus in the closed state, a handle is then withdrawn, which causes the blades to protrude, and at the same time draws them out of the uterus. The blades expand gradually, so as to divide the os internum very slightly, and the os externum completely, thus making the funnel-shaped incision recommended by Simpson, but doing it all at one operation. I believe this to be an improvement on Simpson's instrument, inasmuch as it divides the cervix at one operation, and divides the two sides equally. In using Simpson's instrument, the back is firmly supported during the dividing of the first side of the cervix, but in dividing the opposite side the back of the metrotome slips into the incision already made, and the second side is either divided insufficiently, causing the operation to fail; or it may be divided too extensively, giving rise to profuse hemorrhage, or some of the other accidents that have followed the operation. But Dr. Greenhalgh's hysterotome seems to me objectionable, and that for two reasons. 1st. It attempts to do by mechanical arrangements what should be done by the hand and eye of the operator, that is to regulate the extent and direction of the incision, and it is consequently not adapted to uteri of varying size.

2nd. The bulk of the sheath obstructs the view of the parts to be divided.

The instrument I have used in all my late operations is the one known as Dr. Savage's. It is like a scissors, and consists of two blades, but with their cutting edges directed outwards. In the closed state the blades overlap each other, so that the cutting edge of one is protected by the back of the other, and a sheath is unnecessary. It is introduced into the uterus within the os internum in the closed state, the canal being first dilated with sea tangle, if necessary. The blades are then opened by pressing on the handles, and the instrument withdrawn, dividing the cervix in its passage.

In using this instrument I place the patient in the prone position, and introduce the "duckbill" speculum, which is held by an assistant, I then pass the hysterotome into the uterus, and place my finger on it close to the os, by which means I am able to judge of the progress of the incision, and I divaricate the blades as I withdraw them, so as to divide as much of the cervix as I think necessary, which can be done with great accuracy, as the operation is under the guidance of both finger and eye. After dividing the cervix, I roll a piece of thin lead on a common dressing forceps, and carry it up into the wound, when by opening the forceps I expand the lead, taking care to do so more below than above, so as to preserve the funnel shape of the incision, and withdraw the forceps, leaving the lead in the wound. I then place a piece of cotton wool in the upper part of the vagina, which serves both to keep the lead in its place, and to control bleeding if there should be any. The lead may be withdrawn after a few hours, as there will be no risk of union by the first intention, but in some cases may require to be re-introduced at a subsequent period, to prevent contraction of the cicatrix.

In a paper recently published in the *Lancet*, Dr. Marion Sims advocates, very strongly, the slitting of the os uteri, in cases of dysmenorrhea, and describes the mode he adopts, which is different from any of those I have alluded to. He places the patient in the prone position, and having introduced the "duckbill" speculum, he seizes the anterior lip of the uterus with a hook, so as to steady it and draw it downwards, and then he divides with a scissors the sub-vaginal portion completely through on each side. Having done this, he passes a knife into the uterus, and divides the os internum on each side, but to a less degree of course. The knife he uses in this stage of the operation consists of a long blunt stem, with a small, curved, and movable blade attached at its point. I have not seen the operation performed in this manner, but judging from the description, it does not seem to have any advantage over the less complicated method with Savage's hysterotome.

So much as to the various instruments which have been devised for performing this operation; the objections which have been made to the

*principle* of the operation itself are founded, firstly, on the hemorrhage which it produces. As to this, I believe that with Dr. Savage's or Dr. Greenhalgh's instruments the liability to hemorrhage is very much lessened; and at all events, the hemorrhage can, I believe, in all cases, be fully controlled by plugging the vagina. Another objection, and, perhaps a more serious one, is founded on the liability to the setting in of pelvic cellulitis after the operation, or of inflammation extending to the peritoneum, and giving rise either to acute peritonitis, or (as occurs more frequently) the sub-acute and chronic form of the disease. This objection, is no doubt, a very serious one, but I think that by selecting a proper time for the operation, *i.e.*, when the patient is in good health, and by insisting on a sufficient amount of rest after the operation, the danger is very much lessened. Dr. Gream, of London, has lately proposed a new objection to the operation, but he has surely exaggerated. He states that in a case in which the operation was performed, and the patient subsequently proved pregnant, he found the os at an early period of the pregnancy completely open, and the membrane exposed, as if in the first stage of labour. The result was, that though the lady proved pregnant on several occasions, she always aborted in the third or fourth month. I have attended a lady in several pregnancies, who had been operated upon by Dr. Simpson, and there was no tendency to abort, nor was the os dilated to such an extent as to expose the membranes.

In only one case have I seen the operation followed by hemorrhage, or any inflammatory action. This was a case, in which I operated on the 8th of February last, assisted by Dr. Sawyer. The nurse took on herself, in direct opposition to the orders she had received, to remove the cotton plug and with it the leaden tube, hemorrhage soon set in, and I was sent for about two hours after the operation, when I found considerable bleeding had taken place, I stopped this without difficulty, by cold applications, and re-introduced the plug. The lady made a good recovery, and went to the country on the 20th of February. On the 16th of March she called at my house, and stated she had menstruated without any pain whatever, and on examination I found the os well dilated, and the uterus healthy. On the 26th of March, I was sent for to see her, I found her very feverish, and complaining of severe pain in the back, knees, ankles, and shoulders. She stated that some days before she had been subjected, for nearly two hours, to a very cold draught, and that night had a rigor, followed by the present feverish attack. This state continued with many of the characters of acute rheumatism till the beginning of April. On the 6th of April she was able to sit up, the pains having all disappeared; but in a few days she complained of a fixed pain in the left hip and iliac region, and pain in defecation. No tumour was perceptible in the abdomen, but on making a vaginal examination I found considerable swelling between the uterus and rectum, and



very tender to the touch. This gradually resolved, and the lady left Dublin for the country; and whether the operation is fairly chargeable with the inflammatory attack or not, I will not undertake to say, but it is the only case in which I have seen anything unpleasant follow it.—13th May, 1865.

*Puerperal Tetanus.*—DR. DENHAM read the details of three cases of tetanus occurring after abortion with much hemorrhage; one case of the same affection, after a labour at the full time, which had been complicated with severe hemorrhage and adherent placenta; and a case that occurred in a cow after calving, where the placenta had also been adherent; and made the following remarks:—

When we call to mind the fact that neither Denman, Davis, Bamsbot- ham, Murphy, Meigs of Philadelphia, nor even our esteemed President, Dr. Churchill, make any allusion to puerperal tetanus in their works on practical midwifery; and further, when I state that the subject is not even once alluded to in any of the reports that have emanated from the Rotundo Hospital by Collins, Hardy and M'Clintock, or Johnston and Sinclair, and that, so far as we can ascertain, no case of puerperal tetanus has ever occurred within the walls of that institution, I feel assured you will not think it intrusive on my part when I bring before you these cases of puerperal tetanus.<sup>a</sup>

In Dr. Merriman's synopsis of difficult labours he publishes a list of 10,190 cases in which there occurred 106 deaths, and only one of these is noted as having been produced by lock-jaw.

It was formerly supposed that tetanus was more common among females than males; the reverse, however, is now found to be the case. Out of 128 cases, collected by Mr. Curling, 112 were males, only 16 females.

Professor Laurie, of Glasgow, makes mention of 221 cases; of these 185 were males, 36 females. Of 1,069 fatal cases reported by the Registrar-General of England, 829 were males, 240 females. Another prevalent opinion, namely, that tetanus is more common in this country in the warm than in the cold months of the year is found to be erroneous; thus, in the Registrar-General's report 179 individuals are reported to have died of tetanus in London from 1840 to '47; of these 49 occurred in the Spring quarter of the year, 37 during the Summer quarter, 34 during the Autumn, and 39 during the Winter quarter. There can be no doubt, however, that the disease is more common in warm than in temperate climates.

<sup>a</sup> In the *Dublin Quarterly Journal*, No. XXX., we have a case detailed from the unpublished manuscripts of the late Abraham Collis, where the disease came on a fortnight after delivery, apparently from exposure to cold, and proved rapidly fatal. And in the *Lancet* of June, 1838, Dr. Finucane, of Nenagh, gives a case of placenta previa, in which he turned. Tetanus set in on the fifth day after delivery, and proved fatal in fifteen hours after the attack.

The number of cases in England and Wales has varied, for many years, from 120 to 150 each year. The only cases mentioned by Dr. Simpson as having occurred in his own practice is one where he dilated the os uteri to allow a polypus to descend from the cavity of the uterus. The use of a sponge tent for a few hours opened the os sufficiently. Uterine contractions came on in a few days, and the polypus came away without any further operation. The patient appeared to go on well up to the ninth day, when she complained of stiff and strange feelings about the face. The disease rapidly advanced, and she died in about fifty-five hours after the first symptoms of tetanus appeared. No special lesions were found after death.

Dr. Simpson next gives the result of seven cases, not seen but collected by him, where tetanus came on after abortion. The disease set in at variable periods from fourth to fourteenth day. It was fatal in all, death taking place in one at the end of sixty-one hours, and in another on thirteenth day.

He then gives the history of eighteen cases, in all of which tetanus supervened after parturition at the full period of utero-gestation; three of those are reported as recovering, each under a different plan of treatment. In one case the disease set in on the second day after delivery, in another on the third day, one on the seventeenth day, one at the end of three or four weeks, and lastly, one at the end of seven weeks.

Dr. Waring has published 232 fatal cases of tetanus that occurred after confinement, at Bombay, in three years. The disease appeared at variable periods from first day up to eighteenth. The duration of the disease is also very variable—one patient died in fifteen hours, another lived for ten days.

Of 102 cases at Bombay, published by order of Government, ten set in on the first day, and so on, one on fifteenth and one on the sixteenth day after delivery.

Surgeons are agreed upon the fact, that the state of the wound is as often apparently healthy as it is unhealthy, inflamed or sloughing, and so with regard to the lesions in the uterus, it has been found sometimes healthy and sometimes unhealthy, but in no case was there found any tendency to inflammation of the uterus.

The pathology of tetanus which should direct us in our treatment of the disease is, I regret to say, very obscure. That the disease results from some peculiar condition of the spinal cord produced and kept up by irritation of its substance or its afferent nerves, and that the brain is not involved in the disease, all admit.

The French hold that it is an inflammatory disease of the spinal cord; but how often do we see the spinal marrow inflamed without producing tetanus? and how often do we see tetanus where there is no inflammation of the cord or its membranes?

If you irritate, say by means of a pair of forceps, the exposed spinal cord of an animal recently decapitated you produce spasmodic contraction of the limbs. Why may not some mechanical irritation existing within the spinal canal of a living man have a similar effect. Again, if you irritate one of the spinal nerves of an animal whose head has just been cut off, the muscles of the limb contract spasmodically, those on the side to which the nerve belongs become rigid, and those on the other side also, which proves that the cord has the power or is capable of receiving impressions, through the medium of its afferent nerves, from parts at a distance, and of originating motion in the muscles of the trunk or limbs through the medium of its efferent nerves. These facts, brought out by the experiments of Marshall Hall, throw much light on the pathology of tetanus. We infer from them that the tonic spasm which marks the disease may be caused by a morbid condition of the spinal marrow itself, or of the nerves that belong to it.

In the latter case irritation is set up at the free extremity or somewhere in the course of incident nerves; along these an influence is conducted to the cranio-spinal axis, in which a process or change takes place, whereby an answering influence is reflected to the muscles along motor nerves, and thus the whole circle of action and reaction is run through with the suddenness and swiftness of lightning.

Dr. Marshall Hall calls the disease centric tetanus when the irritating cause operates directly on the spinal cord; when it exists in some part of the body distant from the spinal cord he calls it eccentric tetanus.

We must not forget, however, that a certain predisposition is necessary to render the body susceptible of the disease. But what the irritating cause is we too often know not, and therefore, the treatment, we must confess, is to a large extent, still imperfect, and alas, too often unsuccessful; the acute cases proving, almost invariably, fatal, no matter what the treatment.

Opium has been given to an extent almost incredible; one lady is reported to have taken 40,000 drops in twenty days; in another case an ounce of solid opium was taken, in divided doses, every day for twenty-two days. Most of the cures ascribed to the opiate treatment occurred in the chronic form of the disease, and therefore the question arises were they cures or recoveries?

In France, where the disease is looked upon as an inflammatory disease, bleeding has been pushed to its utmost limit, but with no better success than other remedies. From fourteen to fifteen pounds of blood have been taken in a few days from one patient. Another was bled eight times, and had 782 leeches applied to the epigastrium, and along the course of the spine. Some cases, where there was decided evidence of inflammation, local and general, were much benefited by bleeding in conjunction with other remedies. The warm bath has been pronounced

useful by some, harmless by others, and positively hurtful by not a few. The cold bath has also had its advocates, but with no better success. A tetanic patient in St. Thomas' Hospital was plunged into a cold bath at his own request. All the symptoms disappeared in a moment, but he was taken out lifeless almost immediately. The cold affusion, however, with dry rubbing, is a very favourite remedy, particularly in the West Indies. The late Dr. Todd strongly recommended ice to the spine, a remedy he had found very beneficial in convulsions. Dr. Bush, the celebrated American physician, looking upon the disease as one of debility, recommended bark and wine and spirits in large doses. In such cases nothing like intoxication is produced by it; the system resists the ordinary influence of the alcohol. In one case, related by Dr. Currie, the disease lasted six weeks, and in that time the patient drank 110 bottles of port wine.

Mercury has been tried, and apparently with good effect in some chronic cases; on the other hand, tetanus has set in, in some cases, where the patient was in a state of salivation at the time. Purgatives, fox-glove, musk, prussic acid, and belladonna, have all had their advocates. Tobacco, as you are all aware, has been largely tried in this city, and strongly advocated by the Rev. Professor Haughton; but, I fear, with doubtful success, at least, in the acute form of the disease. It is argued against it, that it acts more on the involuntary than the voluntary muscles, the latter being those especially engaged in the disease, and that death by asthenia frequently occurs in tetanus; therefore the tobacco cannot be a safe or good remedy. Carbonate of iron has been given in large quantities, even as much as two pounds in the day, with success. Oil of turpentine, strychnia, and chloroform have all been highly extolled in turn. The removal of the injured limb, the division of the principal nerve leading to the wound, have also succeeded in arresting the disease, but they have as often failed.

I fear we have yet to look for a specific remedy in this fatal disease, simply because we are yet ignorant of the real nature of the malady or the lesion that induces it.—13th May, 1865.

*Calculus extracted from Urethra by Vaginal Lithotomy.*

DR. CRONYN exhibited a calculus extracted from the urethra of a female in the Rotundo Hospital. It consisted almost exclusively of phosphate of ammonia and magnesia, was extremely friable, and weighed eighty grains. The patient's age was forty-four years, and she had borne twenty-two children. She presented herself at the hospital for relief from a menorrhœa, which it was supposed from her age was consequent upon the change of life, but on examining with the finger the calculus was distinctly felt from the walls of the vagina, and upon the introduction of a sound into the urethra, the peculiar grating sound indicative of calculus was produced. The stone was extracted on the 8th February last. The patient having



been placed under the influence of chloroform, an incision was made through the wall of the vagina, and the calculus extracted by Dr. Denham, with little difficulty. After the operation a syphon catheter was introduced, and retained in the bladder for three or four days; after which, as she did not suffer from any incontinence of urine, the catheter was withdrawn, and in fourteen days the woman left the hospital. The incision was by this time completely healed, and she appeared to suffer no inconvenience whatever. In about a week afterwards she returned to the hospital and submitted to an examination, when scarcely a trace of the incision was to be seen.—13th May, 1865.

## TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.<sup>a</sup>

SESSION 1864-65.

DR. JOHN BAIN, President.

*Cases of Poisoning from Eating the Roots of the *Oenanthe Crocata*.* By JOHN POPHAM, M.A., M.B., Physician to the Cork North Infirmary.

On April 15th, 1865, five boys were brought to the Cork North Infirmary, at three o'clock, p.m., with symptoms of poisoning by the *oenanthe crocata*. They saw the plant growing on the banks of a stream, and mistaking it for field carrots, they all began to eat it with avidity. The effect of the poison was soon apparent. They felt a burning in the stomach and constriction of the throat, with nausea and headache, and one of the party fell down on the bank in strong convulsions. Terrified by this, the others left him, in order to get assistance; but when help arrived, he was found lying on his face, in the stream, quite dead. On being brought to the infirmary, between one and two hours after the occurrence, four out of the five were relieved from the severe symptoms of the poison by emetics and other remedies, but its sequelæ, such as colic pains in the abdomen, loss of animal heat, giddiness, and depression of spirits, remained till the following day. The fifth boy, named Mulcahy, was long in a very precarious state, passing in the interval before his admission through alternations of tetanic convulsions and insensibility, with loss of speech. An emetic of sulphate of zinc was given him, and succeeded in bringing up a piece of the root; the effect was kept up by draughts of mustard and water, which produced a salutary irritation of the tongue and pharynx, rousing him from the lethargic state, as he

<sup>a</sup> These reports are supplied by Dr. David Cremen, Secretary to the Society.

struggled violently against their administration. His symptoms were very critical; face flushed up during his struggles; livid, when quiet; pupils dilated, and insensible; breathing slow and laboured, interrupted by constant sighing and convulsive cough; pulse eighty-four, feeble, and irregular; both the heart's sounds distinctly audible. In order to test the loss of speech, as all our efforts were unsuccessful, I got his mother to speak to him, but for some time without effect, till at last a dim hazy perception of her familiar voice began to dawn upon his mind, and with a spasmodic exertion he jerked out the word "Mamma." After a course of similar entreaties and shakings up, she got him to put out his tongue, but in a very hesitating and tremulous fashion. Considerable hyperesthesia existed in the soles of the feet, the slightest tickling sufficed to rouse him from stupor; and accordingly when his somnolence waxed very profound, we availed ourselves of it, as a therapeutic agent, and he would invariably withdraw his feet with a growl of impatience. When placed in the sitting posture his head used to fall forwards, or backwards, or to the shoulder, as if the co-ordinating power of the muscles was suspended, or the polar force exhausted by the previous discharge on them of the spinal dynamic matter; but when replaced on the pillow, he tossed his head from side to side, accompanied by jactitation of the hands. As the emetics ceased to act the stomach pump was used by Dr. O'Sullivan, house surgeon, and warm water was thrown in, with the effect of bringing off some imperfectly masticated flakes of the root. Strong coffee was given him, which he took willingly, stopping after each mouthful for a second or two. Other remedies, such as sinapisms to the spine and abdomen, cold effusion to head, friction and warmth to feet, and stimulants were used. Roseola was noticed on the abdomen in patches, such as Devergie describes as being occasionally observed in like cases.

Seven o'clock, p.m.—He is in deep sleep, snoring loudly, and moaning, the eyelids spasmodically closed. When slapped on the cheek by the house surgeon, he bounds up indignantly, stammering out in pitiful remonstrance "wisha don't then," and covers up his head impatiently; he is immediately buried in unconsciousness again. No urine passed.

April 16.—More conscious, but still much confused in intellect, and vacant in expression; speech returning, but he takes time to answer a question; pupils less dilated; tongue sore and swollen; pulse 84, rising to 108 on sitting up.

April 17.—Consciousness has quite returned, but all is a blank since he eat the root up to this morning; temper irritable; tongue raw at tip and edges; he says that the quantity of the root which he swallowed was about the top joint of the little finger; the piece thrown up by the zinc emetic was about half that size. He left hospital next day quite recovered.

*Remarks.*—I was able to identify the plant by comparing the leaves and root with the excellent figure in *Sowerby's Botany* (No. 3713). The symptoms just described show the double property which this poison possesses as an acro-narcotic, first of causing local irritation of the mucous membrane in the primæ viæ, and the follicles of the stomach; and secondly, of violently disturbing the equilibrium of the muscular tissues, producing at intervals a powerful discharge on them of polar force, to be succeeded by great exhaustion; along with this irritation of the spinal cord there occurs cerebral congestion, torpor of the organs of sense, and stupefaction of the intellectual faculties. It seems to terminate life by one of two ways; one, by asphyxia, from its tetanic action upon the muscles of respiration, and, perhaps, the heart; the other, and more general way by coma, resembling the mode of death caused by opium in some respects, but differing in the dilated state of the pupil. Its effect upon the fibrin of the blood in destroying its coagulating power is common to it with other poisons.

*Aphasia.*—A symptom occurred in this case, namely, loss of speech, which has lately excited much attention. For some time after the occurrence his utterance was totally abolished, and but slowly returned. The tongue was certainly injured by the teeth during the spasms, but this would produce indistinctness, and not extinction of speech; besides he had the power of uttering sounds or outcries when stirred up actively. There is little doubt that this poison has the property of paralyzing the muscles of the tongue, producing that form of impeded movements of this organ as connected with vocal sounds, which Romberg calls *glossoplegia articulata*. We have had a number of epithets lately coined by the French, intended to express the loss of voice and of speech, such as *alalie*, *aphasie*, *aphemie*, *aphonie*, for the propriety of each of which classical authorities are urged with more zeal perhaps than discretion. If I may be allowed to contribute a mite to this treasury of medical nomenclature, it would be to present the classics of another Greek verb hitherto, I believe, passed over. It appears to me that of all the words in the Greek language denoting modes of speech (and they are multifarious), the verb *φθεγγομαι* applies more than any of the others to the formation by the tongue of *articulate* sounds. If the subject were worth it, we could produce some quotations in proof. The substantive *φθέγξις* is used by Hippocrates, and the privative word *αφθέγξις* would express an inability to enunciate syllables. The English word *aphthenxis*, or *aphthenxia* is as euphonious as many other derivations from the Greek. All the other words appear to me to refer to effect rather than cause. Thus, *aphasia* is certainly loss of speech, but it describes an effect, the cause being emotional, such as the "*vox faucibus hæsit*" of the poet; *alalia* may be applied to an inability to carry on ordinary conversation without losing the memory of words (*amnesia*), as in the case of *Zacharias*,

in *St. Luke*, (chap. i., v. 20). In the present case the difficulty of utterance lasted some hours. The defect exists in a temporary paralysis of the hypoglossal nerve, and the effect probably extends to the olivary bodies, which, according to Schroeder Van der Kolk, are auxiliary ganglions to that nerve. The other form of paralysis of the tongue, Romberg's *glossoplegia masticatoria*, also existed in this case, but with less intensity; he was able to swallow, but not by a continuous effort, but more *per saltum*, and by small portions at a time. This paralysis passed off sooner than the other.

*Case furnished by DR. DAY, Auckland, New Zealand.*

A lady was confined under the care of another practitioner, of her second child; for a fortnight she seemed to be doing well, but the lochia continued. She then began to feel weaker than usual, and complained of wandering pains over the left side of thorax. She did not at first pay much attention to them, but at the end of six weeks the pains became so distressing, that she could not lie on that side.

She then sent for her medical attendant, who had hitherto made light of her complaint; he came, examined her chest, and told her that her lung was diseased.

Her husband was not satisfied, and as I was a personal friend, and had attended other members of the family, he begged me to see her.

I did so, and seeing nothing very urgent, said I would call at night when she had retired, and then examine her minutely, thus saving her the trouble and fatigue of undressing.

I was, however, hastily summoned at 8 p.m., the messenger saying she was dying. On entering the room I thought she was moribund; face ghastly, expressive of the utmost anxiety, the most distressing apnea; pulse thready, and scarcely to be numbered. On examining the chest I was unable to detect any respiratory sound in the left lung, dullness on percussion was evident enough, but not very great as compared with the opposite side, where the respiration was puerile.

No abnormal sound could be detected in the heart; in fact, the distinction between the systolic and diastolic sounds was lost in the rapid fluttering of the organ.

I said her life was in great danger, and her husband at once, at my desire, called in a French physician who had once attended him; he, after a short examination, said it was a lost case. The lady was of a very consumptive family, and he thought that she must have taken cold, and that the symptoms were all caused by a rapid deposition of miliary tubercle.

The immediate treatment, of course, was antispasmodic and stimulant. Next day she was a little better; breathing still quick; pulse 140.

Under supporting treatment she gradually rallied; respiration returned



by degrees to the lung; at the end of three weeks it was fully restored and the pulse were down to 80. She is now in tolerable health.

Could this have been a case of embolism of the left pulmonary artery? Supposing this artery plugged, arterialization would of course cease in that lung; would not respiration also cease as a consequence? When the chest was bared the left side was motionless, while the play of the right was strongly marked.

From first to last she had *no cough*.—*February 21st, 1865.*

*Case of Aneurism of the Arch of the Aorta Bursting into the Pericardium.*  
By THOMAS BLATHERWICK, M.R.C.S., Eng., Staff Surgeon 20th Depot Battalion.

D. C., aged thirty, a soldier of ten years' service, part of which had been passed in India, had enjoyed good health; no record of his having suffered from syphilis. An active, well-proportioned man, of medium stature, face generally pale, and he was prematurely grey. He was a great dancer, with which amusement the disease he was labouring under did not interfere. He never complained to the medical officer during life, but stated to his comrades that he had pain and palpitation of the heart. On entering the door of his barrack, on the 17th February, he was observed to stagger, and then fell back; when I saw him a few minutes after he was quite dead.

*Section cadaveris.*—On raising the sternum the pericardium was seen greatly distended, and the left lung pushed back into the posterior part of the chest. Right lung adherent to walls of chest, by loose bands of adhesion, especially at the apex where it was consolidated; and on cutting into this portion it was found to contain a cavity of the size of a large nut, and several vomicae. Left lung was very slightly consolidated at its apex, which was not adherent, but contained yellow cheesy tubercle in a surrounding capsule of organised lymph. The aorta was then slit up from the centre of its thoracic portion, and it was found to contain much atheromatous deposit, especially at its arch, where portions were seen like small pieces of crushed cancellated bone. The arch of the aorta was much dilated, and at the outer and posterior wall of its ascending portion there was a distinct aneurismal sac, the mouth of which was about an inch and a-half in diameter; and the diameter of the sac from its mouth to its fundus was about two and a-half inches; there was but little fibrin in the sac. By its lower wall the sac was adherent to the pericardium, and here a perforation about three-eighths of an inch in diameter showed where the sac had burst into that cavity and had caused death. On the lower side of the arch of the aorta, nearly opposite the origin of the left subclavian artery, was a large lamina of atheroma, at the edge of which was a lesion of the coats of the vessel extending by a very small opening

into the pericardium, through which blood may also have passed into that cavity.

The features of interest in this case was the man's ability to take active exercise to the last, and comparatively to have suffered so little from this formidable disease. 2nd. The existence of a second, though a minute and inconsiderable, opening into the pericardium. And 3rd. The existence of tubercular disease, which I have never seen in any previous case of aneurism coming under my care.

*Case of Stenosis and Permanent Patency of both the Tricuspid and Mitral Valves; Prominency of Eyeballs.* By JOHN POPHAM, A.M., M.B., Dublin.

The specimen which I beg to exhibit to the Society is one of rare occurrence; it shows the auriculo-ventricular valves at *both* sides of the heart, greatly contracted, and unable to close their respective orifices. The particulars of the case are the following:—

Mary Coffee, aged thirty-one, was a patient in the Cork Union Hospital during seven months, with heart disease, and its sequela, dropsy. The latter affection set in early, and towards the close reached an extent seldom witnessed in like cases. The whole of the areolar tissue of the body was anasarcaous, the neck was so bloated as almost to disappear, the head seeming to be in immediate contact with the shoulders, while the extremities reached the utmost pitch of possible distention. The internal cavities participated in the dropsical effusion; for instance, both pleural cavities showed indications of large quantities of fluid; there was evidence also of hydrops pericardii, and considerable ascites existed. Before the dropsy had gained upon the thoracic cavities a careful examination was made of the heart; dulness extending beyond the sternum towards the right side was found, occupying more than the usual limits of the base of the heart. In the direction of both ventricles a loud and persistent bellows-murmur was heard replacing the first sound; the second sound was distinct but feebly heard. The systolic murmur extended over a wider base than ordinary towards the right, as if it was taken up again and diffused by *consonance*. Proofs of great venous obstruction were seen in the number of large blue veins, much divaricated, which spread over the upper half of the anterior surface of the chest; the jugulars were also turgid and pulsating. The general symptoms were, as may be supposed, very distressing orthopnea, lasting for months; sleep never protracted beyond a few minutes at a time; lividity amounting almost to cyanosis; eyes *prominent* to deformity, their pupils widely dilated; within the chest bronchial râles were audible only in the upper portions, and along the spine. The urine was always scanty, of a sooty colour, s. g. 1025 to 1030, not containing albumen, and loaded with urates. Death was slow in its approaches, and she spoke with great intelligence a few hours before she expired.

I had the assistance of Dr. W. S. Gardiner in making the *post mortem* examination. The chest walls were œdematous and fatty, the lungs were compressed, and pushed upwards and backwards by a large quantity of fluid, their texture was congested and œdematous, the bronchial membrane swollen and red, and the glands much hypertrophied. The pericardium contained from six to eight ounces of straw-coloured fluid. The heart, now before the society, is enlarged, its muscular substance pale, the left ventricle dilated, and its walls much hypertrophied. The mitral valve is contracted to the size of a marble, and a fringe of minute granular bodies covers the free border and surmounts the edge, others being scattered over its tendinous cords. No change in the aortic valves. The left auricle is dilated. Both chambers of the right heart are dilated, and their walls moderately thickened. The pulmonary artery is normal, but the tricuspid valve, like the bicuspid, is studded thickly with the pin-head granulations, its capacity admitting little more than one finger, and permanently open from partial adhesions. The weight of the heart on removing its contents was fifteen ounces; the blood in the large veins was fluid. No examination could be made of the other cavities. A gangrenous eschar was on the right hip.

*Remarks.*—The rarity of a *contracted* state of the right auriculo-ventricular orifice may be estimated from the observations made by Professor Skoda, who had great experience from his connexion with the Vienna hospitals, that stenosis of this opening is “exceedingly rare.” “I have,” he proceeds to say, “never yet observed it in the living. There are some specimens of it in the pathological museums here.”<sup>a</sup> We must take care not to confound this constricted state of the ostium with its opposite or dilated condition, which is frequent enough. In the diagnosis of constricted tricuspid much uncertainty exists. Dr. Stokes is of opinion that “lesions of the tricuspid of any kind are so obscure as in the present state of our clinical knowledge scarcely to justify any positive diagnostics.” What adds to the difficulty is that disease of this valve seldom occurs alone, but is accompanied with change in other valves, and especially in the analogous orifice of the left heart. In such a case the more powerful action of the left ventricle, helps to muffle the abnormal murmurs at the right side. When systolic murmurs occur in both ventricles we cannot apportion how much may be due to each, and we also lose the advantage of comparison. No doubt a systolic sound heard with greater distinctness over the site of the right ventricle, and *not* along the large arteries, awakens the suspicion of a diseased tricuspid. The general symptoms are much more fallacious, but the great retention of blood in the venous tributaries may corroborate the diagnosis. In the above case, the puffy appearance of face, neck, and chest, the staring and

<sup>a</sup> A Treatise on Auscultation by Dr. J. Skoda, translated by Dr. Markham, p. 335.

prominent eyeballs, and the enormous dropsy, can scarcely be regarded as casual adjuncts, but as important aids in detecting the disease.

*A Case of Acute Inflammation of the Right Ovary.* By D. CREMEN, A.B., M.B., T.C.D., Physician to the Mercy Hospital, Cork.

M. S. had an easy labour of her fourth child, on 24th September, 1864. Her health was always good. The lochia on this occasion only continued for one week, while in previous labours they continued for three; on the third day after delivery she was seized with rigor, followed by severe pain in the right inguinal region; this continued more or less severe for three weeks, when after exposure to cold, it became so intense that I was called to visit her, and found her in a feverish state; on examining the abdomen, great pain was complained of on pressure over the hypogastric and right inguinal regions, and an irregular undefined tumour extending in the latter direction was found to exist; and, as the case progressed, became more defined; its length was about six inches, and in breadth about three. The tumour was twice leeches, and constant fomentations and poultices applied; it, however, continued to increase in size, and at length burst through the abdominal parietes, discharging about one pint and a-half of healthy pus, which changed in colour afterwards to a dark brown. She recovered without any bad consequence. Sulphate of quinine was freely given, and her strength sustained.

*A Case of Ileus, terminating in Gangrene of the Muscular Coat of the Small Intestines.* Under the care of E. R. TOWNSEND, M.D., T.C.D.; Licentiate King and Queen's College of Physicians.

Mary Sullivan, aged forty, a servant, unmarried, was admitted into the South Infirmary, Cork, on Saturday, February 11th, 1865. History.—On the Saturday before admission was quite well, and whilst marketing got suddenly ill with severe pain over the region of the liver; she went home and took a cup of hot tea, which she vomited, the pain continuing, accompanied by bilious vomiting; a medical man was called in on the Wednesday following, who gave her a large dose of calomel and opium, and a castor oil and senna draught; this was followed by several large evacuations of liquid feces. The bilious vomiting and pain still continuing she was sent into hospital on Saturday, the 11th of February. On admission was very weak; countenance sunken, and expressive of great anxiety; bowels confined since Thursday night; stomach very irritable, constantly vomiting matter resembling coffee grounds, having a decidedly fecal odour. Tongue red and glazed; abdomen greatly distended, tympanitic, and very painful; but the pain was somewhat relieved by steady pressure; pulse 110, small and feeble; passes urine freely. She was ordered ten grains of calomel and one grain of opium at once, to be followed, in four hours, by five grains of calomel and one



of opium; this latter dose to be repeated towards morning if necessary; to have three pints of warm water injected into the rectum with the long tube, and repeated again in six hours. To be supported with beef-tea and brandy.

February 12th.—No improvement; the enemata did not bring away anything, though the tube was passed up to its full extent; vomiting still continues; had no sleep; enemata to be repeated; to take one grain of opium every four hours, and an ounce every second hour of a mixture containing an ounce of sulphate of magnesia and two drachms of carbonate of magnesia, in eight ounces of infusion of spearmint. This medicine was vomited each time immediately after being taken; no result from the enemata. Galvanism, was then tried without effect.

February 13th.—No action from the bowels; stercoraceous vomiting still continuing; towards evening became quite cold, and suffered from hiccough; pulse weaker; declines to take any nourishment, says it makes her sick; passes urine freely. Ordered two grains of calomel and one of powdered opium in a pill every four hours.

February 14th.—Appears better than last night; pulse something stronger; slept a little towards morning; still unable to bear anything on her stomach; abdomen tympanitic. Galvanism was again applied, but caused so much pain it had to be discontinued; to take three grains of calomel and one of powdered opium every third hour; to have another enema with a drachm of tincture of opium added; to be given with the long tube; no result; to have brandy, wine, and arrow-root in small quantities.

February 15th.—Fifth day since her admittance, abdomen softer, but still painful; can bear pressure, but cannot bear to be moved; pulse small, 130; she now, for the first time, noticed that she could pass wind from the bowels; this she continued to do in large quantity; has frequent hiccough; says she cannot see anybody; pupils contracted. Ordered five grains of Corban's extract of colocynth, with one grain of opium every four hours.

February 16th.—Pulse stronger, 110; abdomen much softer; gets up to the night chair frequently, but only passes wind in large quantities; kept down a little arrow-root; sight quite gone; pupils contracted; not drowsy at all. Ordered sixteen grains of calomel, half a grain of strychnine, three drops of spirits of wine, eight grains of powdered opium, and twenty-four grains of extract of Barbadoes aloes; to be made into eight pills, one to be taken every fourth hour. To continue brandy and arrow-root.

February 17th.—Abdomen still soft; complains of great pain if she is moved; stomach not so irritable; pulse 120, very weak; no action from the bowels.

February 18th.—Pulse 120, very weak; abdomen flaccid; vomiting

has quite ceased. To continue the strychnine and aloes pills, omitting the opium, and to take one ounce of infusion of senna of double strength, with two drachms of compound tincture of senna. In about four hours after taking the first of these pills and the draught she got out of bed and passed a large quantity of feces, containing much solid matter; the quantity was so great as nearly to fill the night chair; the bowels were moved again twice, largely, during the night.

February 19th.—Still complains of pain in the abdomen, but cannot now bear to be touched; is very feeble; bowels moved three times to-day spontaneously; took a pint and a-half of chicken broth with eight ounces of brandy, which were retained; sickness of stomach quite subsided.

February 20th.—Very weak, evidently sinking.

February 21st.—Very weak, but takes her nourishment well, consisting of brandy eight ounces, wine six ounces, chicken broth two pints; she continued to sink, and died at one o'clock on the morning of the 22nd. The sphincters became relaxed before death, and she passed involuntary stools under her.

This case was diagnosed to be one of ileus, and not of intus-susceptio, from the fact of the bowels having been freely opened by purgative medicine after the first setting in of the symptoms of obstruction, and from her being thereby only partially relieved; next day the pain increased, the vomiting returned, and became dark coloured and offensive, having a decidedly fecal odour. On her admission into hospital she was very weak; pulse 110, small and feeble; countenance sunken; belly distended and tympanitic, but pain not increased by pressure; no symptoms of peritonitis; said the pain was rather relieved by slight pressure over abdomen; stomach very irritable, rejecting everything immediately on its being taken. As she passed urine freely, and in large quantity, I concluded that the obstruction was in the lower part of the intestinal canal, and, from the fecal odour of the vomited matter, somewhere near the cecum or the lower part of the ileum; that it was not below the cecum was evident from the fact of the long tube having been several times passed up its entire length, and a large quantity of fluid injected each time, without bringing away any fecal matter. The treatment at first was by calomel and opium, then galvanism was tried without effect. On the Wednesday after her admission she said she could not see; her pupils were contracted, showing that she was fully under the influence of opium, but she was not at all drowsy; towards evening she began, for the first time, to pass wind from the bowels, showing that the canal was pervious, and leading me to hope that the spasm was relaxed to a certain extent. The colocynth and calomel not having produced any effect, I thought that the muscular coat of the bowels might be in a paralyzed state, and unable to contract sufficiently to expel its contents, and therefore determined to

try the effects of strychnine in combination with aloes, calomel, and opium, these pills she took for twenty-four hours without any action from the bowels, when both pain and vomiting having ceased I omitted the opium, continuing the strychnine, aloes, and calomel, and ordered a draught of strong infusion of senna. In four hours after taking the draught and one of the pills the bowels were moved largely, and several times after during the night, giving rise to a hope of her recovery; but it was a delusive one, as she sank gradually, and died on the 22nd instant, three days after the obstruction had given way. The *post mortem* was made eighteen hours after death, the following are the results:—Rigor mortis well marked, some commencing decomposition over right groin and down right thigh; on opening the abdomen a quantity of intolerably fetid gas escaped; the peritoneum was quite healthy, no signs of inflammation of it; no recent lymph, except at the femoral ring, where there was a small quantity of recently effused lymph cementing an enlarged gland to the ring; this gland occupied the entire ring; the small intestines were very much distended, and enormously increased in calibre; both the lower part of the jejunum and the entire extent of the ileum were both quite black and gangrenous, so soft as easily to be torn through. This gangrene appeared to be confined to the muscular coat and to the peritoneum immediately over it; the mucous coat was softened, but not otherwise altered, and not gangrenous; it was intensely congested; the colon was nearly empty, contracted and pallid in appearance; it was quite healthy, as were all the large intestines. The uterus was converted into a mass of scirrhus, with a large scirrhus tubercle, about the size of a small egg, attached to the fundus. It was clear from the results of the *post mortem* that the case was one of inflammation of the muscular coat of the ileum passing into gangrene; the action of the bowels was probably due to the relaxation of spasm caused by the gangrene, and was not due, as I at first supposed, to the effects of the strychnine. The case resembles some of those recorded by Dr. Abercrombie in his admirable work on the diseases of the stomach and abdominal viscera. Had the case come under my care earlier I think bleeding, either generally or locally, would have been of service, as the mercury and opium did not check the progress of the inflammation although given to the fullest extent; but on her admission into hospital she was too weak to allow of it; and having been nearly a week ill I thought the time for bleeding had gone by. The absence of peritoneal tenderness prevented my applying leeches to the abdomen; this absence of pain on pressure is, I think, curious, when there was such an extent of inflammation in the muscular coat of the small intestines. The duration of the case is also remarkable.

## CLINICAL RECORDS.

*Cases Treated in the Mater Misericordiae Hospital.* By T. HAYDEN, M.R.I.A., F.R.C.S.I., &c., Physician to the Hospital.

I.—*Partial Hemiplegia, with Convulsions, in a Child Twenty-seven Months Old; Recovery.*

Read, a boy twenty-seven months old, was admitted into hospital, Feb. 2, 1865; has been subject to "fits" since he was seven months old; during these fits he has a tendency to fall forwards and to right side, so as to strike his forehead on that side against the floor, if not supported in time; there is likewise, during the fits, agitation of the limbs and slight frothing at the mouth, with remarkable tremor or quivering of the features, and traction of the face to the left side

The approach of these attacks, of which there is one about every hour, was indicated by a tendency to droop, and irritability of temper. Left leg and arm are weaker than right; he is barely able to stand without support, and never uses the left hand; there is no strabismus; bowels free and appetite good; sleeps well, save when disturbed by fits; is well nourished, and rather irritable. Treatment consisted in gr.  $\frac{1}{4}$  extract belladonna at night, blister to nape of neck, iodide of potassium, and aperient powders (pulv. scammon. c. gr. iii, calomel gr. ii, M. ft. pulv. P. r. n. sumend). This treatment was pursued for some weeks without much benefit. An issue was then made in nucha, and iodide of potassium and aperients continued with marked benefit. For fourteen days subsequently he had no *major* fit; he had, however a *minor* one every day. One of these I witnessed on the 11th of April. The boy was somewhat irritated, and suddenly his mouth was drawn to the left side; the left eyelids quivered, and the globe of the eye was inverted; there was no alteration of the pupil; finally, the eyelid drooped, the spasm ceased, the child became lethargic for a few minutes, and then completely recovered and became lively as usual.

On the 16th of April he had an attack of unilateral (left) convulsions lasting over two hours; this was repeated on the 17th, and witnessed by me. The child was apparently conscious, but lay motionless; features heavy; pupils rather dilated, but equally so; left side of face was twitched regularly, thirteen times in the minute, the angle of the mouth being drawn, during each spasm, to left side, and eyelids closed an equal number of times. The nurse reports that at the commencement of the attack the left leg moved synchronously with the face, but not the arm; pulse, 120, weak; face neither pale or flushed.

R̄. Syrupi papaver, ether. sulph. a.a. gts. iii, aquæ 3 iii., statim.

R̄. Hydrarg. c. cretâ gr. iiss, pulv. antimonial gr. iss, m., ft. pulv. ter indies sumend. Pulv. aperiens sicut antea, and blister to vertex.



From this date the child had no fit of any kind for three weeks, at the end of which time he was discharged from hospital.

I regarded this as an example of disease, probably of a strumous character, as there was a family history of struma, located at the base of the brain, and implicating the *right* lateral lobe of the cerebellum and the motor tract.

That the disease was located on the right side was inferred from the paralysis of the arm and leg being on the left; that the right lateral lobe of the cerebellum was implicated I concluded from the tendency to rotatory movement towards that side during the fits; and that the seat of the disease was *below* the decussation of the *portio dura* of the seventh pair, was to be deduced from the fact that the paralysis of the face was on the right side, whilst that of the extremities was on the left. The successful treatment by counter-irritants, iodide of potassium, small doses of mercury, and active aperients, in a case of apparently so formidable a character, and such long standing, affords encouragement.

## II.—*Scorbutic Hemoptysis; Recovery under Treatment by Lemon Juice and Quinine.*

Henry Byrne, aged 45, cab-driver, admitted into hospital, June 9th, 1865; spat blood fourteen years ago, and occasionally since, a streak; has repeatedly bled from nose and gums. On this morning, June 9th, spat up a large quantity of dark blood; pulse, 72; dark blue line on margin of gums; moist râles universal over both lungs, loudest and most numerous in bases; pulmonary resonance and heart's action and sounds normal. *R.* Acid gallic, gr. v. 3tia. quaque hora.

June 12th.—Bled rather freely from nose last night, and continued to expectorate dark blood. *R.* ʒ ss. succi limon. ter in die; stop gallic acid.

June 13th.—No change; bled from nose and spat blood as previously; continue lemon juice.

June 15th.—Fine crepitus over lower half of left lung anteriorly, mixed with loud râles; but this and all other parts of chest resonant. *R.* Quinæ sulph. gr. xxx, acid sulph. dil. ʒ i., aquæ ad ʒ viii., st. coch. mag. ter in die.

June 20th.—Spat some florid blood mixed with frothy mucus; continue medicine.

A few days previously, in consequence of a suspicion I entertained of the existence of latent or central pneumonia, arising from the occasional resemblance of the sputa to those met with in that disease, I examined some of the expectorated matters microscopically; these were found to consist only of blood-cells, more or less disintegrated, mucus-cells, and epithelium—no compound granule cells, tube, or cell-cast, or other evidence of inflammatory exudation.

June 25th.—No blood whatever in expectoration, which is mucopurulent, since last report.

June 27th.—A little dark blood in expectoration.

June 30th.—No blood expectorated since 27th; expectoration mucopurulent and copious; patient walks about; sleeps, and takes his food well; is strong and in good condition; no night sweats or other evidence of phthisis; pulse did not rise above 80 during patient's stay in hospital; he lay on either side indifferently; no pain in any part of chest; loud bronchial râles continue over entire of both lungs. Diagnosis:—Bronchitis, with pulmonary hemorrhage, resulting from scorbutus.

I have seen this man within the current month (October) in comparatively good health, and earning his bread by driving, as before.

### III. *Aggravated Dysmenorrhea; Exploration of the Uterus with the Endoscope; Cure.*

Anne Crolly, aged thirty-five, unmarried, settled in England many years, and has worked in a cotton factory; of full habit, rather pale and flabby, and apparently in good health, admitted June 23, '65.

*History.*—Two years ago had needing, followed by copious hemorrhage from the womb, which was accompanied by discharge of "clots;" this occurred suddenly, and continued eight months without intermission, but was not attended with pain. Has been repeatedly under medical treatment in England, and took mercury to salivation.

Bleeding ceased for five weeks; it then returned, and has continued to the present time; it is now, however, attended with pain, which she describes as being of a "squeezing" kind, and accompanied by "forcing."

Os uteri is patulous, and plugged with glairy mucus; its lips are congested, and on the slightest irritation florid blood flows freely from them.

Examined with Dr. Cruise's endoscope; the lining membrane of the uterus presented streaks of vascular engorgement, like the conjunctiva in a state of chronic inflammation; in several situations, also, the mucous membrane was seen to be rough and granular. It was touched, through the endoscope, with a twenty grain solution of nitrate of silver, and the *blanched* surface so treated was subsequently distinctly visible through that instrument.

Treatment consisted, in addition to the above, in repeated leeching of anus, warm hip baths, injections of sulphate of alum and oak bark, mild aperients, and vaginal suppositories, composed of acetat. plumbi, gr. x, extract opii. aquæ, gr. i, unguent hydrag. ʒi. M.

July 5.—Greatly improved; no bleeding for several days, and pain in breasts, which, when hemorrhage was troublesome, had been urgent, no longer exists.

Diagnosis—Dysmenorrhea from uterine congestion.

July 19.—Examined to-day; os uteri much less patulous; it is now pale, and a transparent glairy mucus oozes from it; no hemorrhage or pain for last three weeks; breasts still tender to pressure, but pain removed, partly by means of aconite and chloroform liniment, and gr. i of valerianate of zinc, ter in die; but mainly, no doubt, by the treatment directed to the uterus. Discharged this day.

With the assistance of my colleague, Dr. Cruise, the interior of the uterus was examined by means of his endoscope, by Drs. Churchill and William Stokes, jun., at a time when vascular congestion existed in a high degree.

There can be no doubt that the practical value of the endoscope was illustrated in this case, as without the use of it; although one might infer from the symptoms the condition of the interior of the womb, it would have been impossible to have the advantage of *ocular* demonstration of this, and to have directed local treatment with the eye.

September 13, '65.—I received a letter from Crolly's mother to-day, from Bolton, England, thanking me "for saving her daughter's life," &c., "after she had been treated to no effect in England."

I mention this letter, which was not in any way solicited, for the purpose of showing that up to that time the girl had continued well.

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*Artificial Tooth and Plate Swallowed during Puerperal Convulsions; Gastritis; Death.* Reported by E. T. BURTON, Birmingham.

Elizabeth Scotney, aged twenty-seven, about six and a half months pregnant of her first child, complained of distracting pain in the forehead, at six o'clock, p.m., 28th May, having dined early in the day off part of a boiled leg of beef and a large quantity of new potatoes. The pain continued without intermission till the same hour next evening, when she was suddenly seized with epileptic convulsions. A surgeon in the neighbourhood was called in, and he ordered her a mustard plaster to the back of the neck, telling her friends, when he left, to let him hear of her in three hours. At the expiration of that time he was informed that she had had twelve similar attacks, when he refused to see her again. At her husband's request, I saw her at twelve o'clock, and found her almost comatose, the features distorted, and the tongue dreadfully lacerated; pulse 120; great heat of forehead; fetal heart distinctly heard; os uteri perfectly closed; bladder nearly empty; no albumen present in the urine; bowels rather confined. Ordered a fly blister to the back of the neck, three leeches to each side of the forehead, a purgative enema—to have internally, if the enema did not act, one minim

of croton oil with ten grains of calomel. Saw her again at five o'clock, a.m., 30th—pulse same; had had only five fits since twelve o'clock, and but two of these occurred after depletion by the leeches and vesication by the blister. On vaginal examination, found the cervix uteri low in the pelvis, the os soft, dilated to the circumference of a florin, and the membranes unruptured. I at once let the liquor amnii escape, which gave immediate relief. Bowels shortly after were freely moved. I then sent for Mr. Scofield, of Highgate, when we delivered her by craniotomy of a fine-sized healthy boy. During the operation, which occupied some ten minutes, she had two slight fits accompanying uterine action. Delivered at 6 a.m., and slept till 10 a.m., when she awoke, and had another attack, and two more less intense, within an hour. From this time she seemed to mend, consciousness gradually returning; pulse 90, but weak. Ordered, at first, a dessert spoonful of the egg-and-brandy mixture every twenty minutes, and afterwards but every hour; beef-tea and milk *ad libitum*. Bowels freely moved, and water passed regularly. Saw her again at 10 p.m.—Considerably improved, attempts to speak, but unable to articulate, from the swollen state of the tongue. 31st.—Pulse 100; skin hot and dry; considerable thirst; stopped the brandy, and ordered some bark, with ammonia; and as there was slight tenderness over the abdomen, to have a linseed poultice. 5 p.m.—Vomiting suddenly set in. To have three minims of prussic acid (P.L) with some ammonia, every hour, and a large mustard plaster over the heart and stomach, also to swallow some ice. When this treatment failed to allay vomiting, gave her creasote, but without success. Patient gradually sank, and died at 2 a.m., June 1.

*Post mortem examination.*—Lungs, liver, and kidneys all healthy; heart pale, and containing a small quantity of thin blood. No solid food in the stomach, but about twelve ounces of fluid, and an artificial tooth, with gold plate attached, which the patient had evidently swallowed during one of the fits, as the friends state that they saw it in her mouth when she was first seized. Joined to the plate (which was one and a half inches long by half an inch broad) were five sharp hooks to fasten on the adjoining teeth, and so placed that however the plate was turned one of these hooks came uppermost and grappled to the rugæ of the stomach. I found it at the pyloric orifice, and in its vicinity a considerable patch of inflammation, as also in five other parts of the stomach. Very slight inflammation of the peritoneum, but of the fundus uteri and about the broad ligaments considerable; otherwise the uterus was healthy.





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